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PURE YEAST AND ITS RELATIONS TO BREWING OPERATIONS.

THE practical application of *pure yeast* in the brewery must be considered to date from the year 1883, when Professor Hansen first introduced a pure cultivation of yeast into the Carlsberg Brewery at Copenhagen. Although many years previously a method had been described by Pasteur for the purification of brewers' yeast, this method had for its object merely the removal of bacteria, which are always present in commercial yeast. In fact, at the time when Pasteur made his experiments, it was not known that many of the so-called "diseases" of beer are caused, not by bacteria, but by certain races of yeast, which are now commonly spoken of as "wild" types, and it is indeed to Hansen that we are indebted for the discovery of this fact. Hansen found that some of the commonest "sicknesses" to which beer is subject are caused by these objectionable races of yeast and that the development of these was actually encouraged by the treatment proposed by Pasteur. With the object, therefore, of eliminating these and all other injurious organisms, the Danish *savant* not only devised a method by means of which it was possible to prepare absolutely pure cultivations of the different yeasts, but in conjunction with Captain Kühle he also succeeded in devising an apparatus for

the propagation on a commercial scale of his pure yeast cultures.

Hansen's system consists in the employment of a single and carefully selected species of yeast, the absolute purity of which is ensured by the fact that it is grown from a single cell.

Before a pure yeast can be safely introduced on a large scale into the brewery it is, however, necessary to make preliminary trials on a small scale, for it is found that many of the innumerable representatives of *Saccharomyces cerevisiæ* differ widely in their properties and that a variety which gives good results in one brewery may prove unsatisfactory in another. This fact alone explains many of the unsatisfactory results, which were at first obtained with pure yeast, and in consequence also much of the opposition which was brought to bear on the Hansen system.

The Carlsberg Brewery, in which Hansen made his experiments, is a low fermentation (lager beer) brewery, and it is therefore natural that the first extension of his system of pure yeast should have been in breweries of the same kind. The number of lager beer breweries in which pure yeast of single race is employed has steadily increased from year to year and is at the present time very considerable ; and the success of the application of such yeast to low fermentation brewing is now very generally acknowledged to be an accomplished fact.

The main advantages accruing from the employment of pure yeast of single race are (1) the possibility of effectually guarding against all the troubles which arise from the presence of foreign organisms, (2) that the brewer can always depend upon having a yeast constant in its properties and upon obtaining a fresh supply of the same yeast as soon as his store yeast shows signs of contamination.

It is obvious that the above advantages should apply with equal force to the high fermentation system of brewing adopted in this country, and to a considerable extent also in some continental countries, provided at least that a single race of yeast is in this case also capable of exercising the necessary functions of ordinary brewers' yeast, which is

known to be in the main a mixture of different races of yeast.

The employment of single race yeast in high fermentation breweries advanced more rapidly abroad than in this country, and the Hansen system soon gained some prominent advocates in this branch of the brewing industry. In this country, on the other hand, brewers have been slow to benefit by Hansen's investigations and it is only in very few breweries that a fair trial of pure single race yeast has been made. An argument that is frequently urged against the application of the Hansen system in high fermentation breweries is that ordinary brewers' yeast contains different races, certain of which bring about the principal or primary fermentation, whilst others induce the secondary or cask fermentation, and that a single species of yeast is incapable of exercising this double function.

It is contended, and no doubt with truth, that the success of single race yeast in low fermentation brewing cannot be regarded as in any way proving that single race yeast is capable of giving equally good results in high fermentation breweries. In the first case the fermentation is carried out at a very low temperature and the beer is also stored at a low temperature, and under these conditions sufficient carbonic acid gas will be retained to render the beer brisk. In the case of high fermentation, on the other hand, much higher temperatures prevail and a true cask fermentation becomes absolutely necessary. Otherwise the beer will be flat and will be in a condition in which it is very readily attacked by bacteria and wild yeasts, which can never be absolutely excluded under the present conditions of brewing. It is clear, therefore, that the question of the application of Hansen's system to high fermentation brewing must be considered as distinct from that of its employment in lager beer brewing.

With regard to the actual evidence for and against the use of single race yeast in English breweries it will be seen, from what has been stated above, that the facts which have been established can refer only to a very limited number of breweries.

In the first place we have the evidence of Messrs. Brown and Morris, who conducted a number of experiments, extending over a considerable period, at Messrs. Worthington's brewery, at Burton-on-Trent. As no account of these experiments has been published, it is, however, not possible here to do more than record the negative result arrived at. In speaking of their experiments, Mr. Horace Brown recently stated that "up to the present he had not been able, with pure yeast, to produce a beer which would pass muster in every respect".[1] Their chief difficulty appears to have been due to the fact that they did not succeed in finding a yeast which gave a satisfactory cask fermentation, and in consequence the beers soon became unsound.

On the other side we have the experience of Mr. W. R. Wilson,[2] at Messrs. Combe & Co.'s brewery, London, and of Mr. C. F. Hyde and the writer at Chester's[3] brewery, Manchester. At the former brewery pure single race yeast has been employed for over eighteen months and at the latter brewery for nearly the same length of time, and in both cases with marked success.

It has been argued by the opponents of the Hansen system that where a beer fermented with a single race yeast had undergone secondary fermentation, this has been due either to the formation of maltose through the diastatic action of the hops[4] added to the finished beer at racking, or to wild yeasts which it is impossible to exclude. Wilson has shown, however (*loc. cit.*), that beer which is run into sterilised bottles direct from the Hansen apparatus undergoes a good secondary fermentation and that the latter is brought about by the original yeast and in the absence of all wild yeasts. In the case of Chester's pure yeast it has also been proved (Miller and Hyde, *loc. cit.*) that the

[1] *Trans. Inst. Brewing*, vol. vii., p. 101.

[2] *The Brewers' Journal*, 1892, p. 527, 1894, p. 49; *Trans. Inst. Brewing*, vol. vii., p. 87.

[3] *Trans. North. Eng. Inst. Tech. Brewing*, vol. iii., p. 39.

[4] Van Laer, *The Brewers' Journal*, 1893, p. 658.

secondary fermentation is caused by the pure yeast started with and that the traces of wild yeast which cannot be excluded in practice play but an insignificant part. This is illustrated in the following table, which represents one of a considerable number of similar experiments. It will be seen from the column headed "specific gravity" that the secondary fermentation was really complete after nine days' forcing, that the wild yeast, which could be detected by the microscope, had produced no appreciable effect on the gravity of the beer even after fifteen days' forcing, and that it was only after twenty-five days' forcing that its effect in lowering the gravity was noticeable. In these experiments the proportion of wild yeast which gained admittance through the unavoidable contact of the wort and beer with the atmosphere was too small to produce any harmful effect and the beer remained sound to the end.

BEER PRODUCED WITH SINGLE RACE YEAST.

	Specific Gravity.	Appearance of Sediment.
Before forcing - -	1·0190	
After forcing 6 days at 33°C. -	1·0112	Culture yeast fairly healthy; no wild yeast
After forcing 9 days at 33°C. -	1·0100	Culture yeast granular and shrivelled; a few wild yeast cells.
After forcing 15 days at 33°C.	1·0100	Culture yeast shrivelled; many wild yeast cells.
After forcing 25 days at 33°C.	1·0080	Culture yeast shrivelled; many wild yeast cells.

It should be stated that no hops were added to the samples of beer experimented with so that there is here no question of diastatic action.

An important paper on secondary fermentations has been recently contributed by Professor Van Laer,[1] who advocates the employment of "composite yeasts," that is to

[1] *Trans. Inst. Brewing*, vol. vii., p. 55.

say of mixtures of different varieties of yeast. Van Laer was among the first to introduce the Hansen system into high fermentation breweries in Belgium, and after meeting with a varying degree of success he has given up single race yeast in favour of his composite yeasts. He has not yet published the method by which he prepares his pure composite yeasts but he appears to be largely guided by the composite nature of ordinary commercial yeasts. The purity of his composite yeasts would seem to apply in the main to their freedom from bacteria, although dangerous wild yeasts are also presumably excluded. But many varieties which are generally regarded as wild yeasts, and which are acknowledged by Van Laer to be highly objectionable when used alone, are regarded by him as necessary, in that they "complete or perfect the effect of other yeasts" (*loc. cit.*, p. 64).

In discussing secondary fermentations Van Laer distinguishes between (*a*) beers with dry hops added at racking and (*b*) beers to which no dry hops have been added. In the former case the diastase, which Brown and Morris have shown to be present in hops, exerts a hydrolytic action on the carbohydrates present, producing maltose which readily ferments, and the secondary fermentation is thus at once explained. This is illustrated by the following table taken from Van Laer's paper :—

No.	Nature of the Yeasts.	Rotation at Time of Bottling.	Rotation after the After-fermentation.	Difference in Rotation.
1	E_1	19·3	17·8	1·5
2	$E_1 + E_4$	19·0	16·2	2·8
3	$E_1 + E_5$	19·1	14·9	4·2
4	$E_1 + E_7$	19·7	13·2	6·5
5	$E_1 + E_8$	18·2	13·8	4·4

In these experiments equal quantities of the primary yeast E_1 were added to five samples of the same sterilised wort and to four of these samples (Nos. 2, 3, 4 and 5) were further added traces of the yeasts E_4, E_5, E_7 and E_8. The worts were fermented at a temperature of 20°C. and the

resulting beers were decanted into bottles containing a small quantity of dry hops. Six days after bottling the beer No. 5 was in a violent fermentation; No. 4 and No. 2 showed a very slight secondary fermentation whilst the other beers were flat. After eleven days No. 5 and No. 4 were fermenting excessively, No. 2 and No. 3 very slowly and No. 1 was still flat. The fermentation of No. 2 and No. 3 became more marked during the following days but did not reach the intensity of No. 5 and No. 4. The beer No. 1 only showed a slight fermentation after a month. At this point the rotatory power of the beers was determined and the deposits were microscopically examined with the results shown in the table.

These and other similar experiments lead Van Laer to the following conclusions:—

"(*a*) When dry hops, with a proper diastatic power, are added to a beer at racking, and if the after-fermentation is not opposed by any mechanical cause, it will always show itself after a more or less extended period, irrespective of the number or the nature of the yeasts used at pitching.

"(*b*) If the principal yeast is mixed with even very minute quantities of yeast found in the deposits of beers that have undergone an after-fermentation, the latter always shows itself quicker and with a greater intensity than when the beer has only been fermented with the principal yeast.

"(*c*) The intensity of the after-fermentation depends upon the nature of the yeasts mixed with the principal yeast. Some give rise to an excessively violent fermentation, with production of very light cells, which remain a long time in the beer. Others produce a more moderate secondary fermentation; among the latter Saccharomyces some very heavy caseous yeasts are found, which the carbonic anhydride only raises with difficulty. It is usually these which produce normal secondary fermentations."

The question of secondary fermentation in beers, to which no dry hops or no diastatic agent has been added, is, however, of greater interest and importance in connection with the employment of pure yeast. It would in fact appear from experiment No. 1, in the above table, that

where the secondary fermentation is dependent solely upon the diastase of the hops, the result is really very far from satisfactory, and that neither the diastase of the hops nor the wild yeasts which they also contain can be depended upon to give more than a very feeble and unsatisfactory after-fermentation.

It has been shown by Morris and Wells[1] that when a wort is fermented by a single race of yeast "a certain definite limit-attenuation" is attainable and when this is reached the fermentation will cease. The limit-attenuation varies with different races of yeast. When a mixture of yeasts is employed a limit-attenuation is also attained and in this case "corresponds with the limit-attenuation of the lowest type" present in the mixture (Van Laer, *loc. cit.*, p. 78).

It is generally acknowledged that there are many yeasts which are unable to bring about any secondary fermentation. Such yeasts ferment the whole of the carbohydrate matter which they are able to decompose during the primary fermentation; the limit-attenuation is thus attained, and in the absence of other yeasts capable of carrying the fermentation further, or of diastase, which gives rise to the formation of maltose and thus renders it possible for the original yeast to recommence fermentation, the beer will remain flat. On the other hand it is also acknowledged that races exist which readily ferment the free maltose of a wort but *only slowly decompose certain other constituents* (Van Laer, *loc. cit.*). This latter property is necessarily of importance in the selection of a suitable race of yeast which has to effect both primary and secondary fermentation, and although, as shown by Van Laer, it may not be sufficient to ensure a satisfactory result in other respects, it affords an explanation of the fact that it is possible to obtain a secondary or cask fermentation with pure yeast of single race. In the case of such yeasts, the limit-attenuation can, at any rate in many cases, be rapidly reached if a high temperature (*e.g.*, 30° C.) is maintained during fermentation, but under the normal conditions obtaining in a brewery the limit is, at

[1] *Trans. Inst. Brewing*, vol. v., p. 133; vol. vii., p. 86.

least in the case of certain yeasts, only slowly attained. Van Laer, however, regards as true secondary fermentations only such as are produced by means of composite yeasts, and when an after-fermentation is obtained with the help of a single race yeast he considers it to be "artificial" and as the result of arresting the primary fermentation at a certain point. He regards it in fact as the completion of the primary fermentation. When, however, all legitimate attempts to carry the primary fermentation to the limit-attenuation fail, it is difficult to understand how the cask fermentation, which must in consequence necessarily take place, can be called artificial. In the case of the single race yeast employed at Chester's brewery, the writer has recently determined the limit-attenuation and found it to coincide with the attenuation attained at the end of the secondary fermentation and to be considerably lower than is reached at the end of the primary fermentation, thus affording additional proof that the after-fermentation is independent of diastatic action or of accidental contamination by foreign yeasts.

Van Laer describes a number of interesting experiments illustrating the secondary fermentations obtained with mixed yeasts of known composition. The following example will suffice to show the effect produced by a trace of a low type yeast when added to a primary yeast. Two samples of the same wort were fermented, one with the yeast E_2, the other with the same yeast with the addition of a trace of the yeast E_5. After fermentation the resulting beers were decanted into sterilised bottles, which were then hermetically closed and exposed on the forcing tray to a temperature of 30°C. After eight days the rotatory power of the beers was compared with that found at the time of bottling, the difference indicating the extent of the secondary fermentation.

Yeasts.	Original Rotary Power.	Rotation after Forcing.	Decrease in Rotation.	Remarks.
E_2	19·4	19·3	0·1	Flat.
$E_2 + E_5$	19·0	15·7	3·3	In good condition.

If the so-called secondary yeasts were present in large proportion, *e.g.*, twenty-five per cent. of the total yeast, no secondary fermentation takes place (Van Laer, *loc. cit.*). In this case they are able to exert their full activity during the primary fermentation, and when this is finished the limit-attenuation of the mixed yeast is attained and consequently no after-fermentation is possible.

The above and other similar results obtained by Van Laer conclusively proved that in his experiments the secondary fermentation was brought about by the so-called secondary yeasts. The fact that the latter types are able to carry on the fermentation of casked beer and thus take part in the secondary fermentation has, however, long been known to brewers, and the fact that in the above experiments the secondary fermentation is brought about solely by these lower types merely proves that under the conditions adopted by Van Laer, his principal yeast (E_2) had reached its limit-attenuation with the completion of the primary fermentation.

Interesting as these laboratory experiments are they cannot be regarded as proving that the same holds good in practice. Ordinary brewers' yeast certainly contains wild or secondary types and these are seen to develop during the secondary fermentation, but that they are the sole cause of the latter and that certain of the principal yeasts do not also play a prominent part is not proved by Van Laer's experiment.

The chief objection which has been raised to the employment of composite yeasts is that it is practically impossible to maintain that constancy of composition which is necessary in order to ensure uniform results in the brewery. In a mixed yeast one variety is liable to develop with greater vigour than another, so that after a few fermentations the yeast will have a different composition and consequently different properties from those which it originally possessed. According to Van Laer this is by no means necessarily the case, and constancy of composition may be maintained, owing to the higher fermentative energy of one species over another being counterbalanced by a more

feeble coefficient of reproduction. This, however, requires further confirmation, for according to Jörgensen,[1] Van Laer's composite yeasts do not maintain the constancy of composition claimed for them. In connection with this it is necessary to prove not merely that the different varieties of primary yeasts maintain their relative proportions during a series of fermentations but that the traces of secondary yeasts which Van Laer maintains to be necessary likewise remain constant.

It would appear, for instance, highly probable that the latter types being present only in very minute quantity might readily become crowded out during the vigorous primary fermentation in which they are acknowledged to play but an insignificant part, and that the wild yeasts present in the atmosphere will quickly take their place. If this should prove to be the case, it is evident that the character of the secondary fermentation will be largely a matter of chance and will depend upon the number and nature of the wild yeasts present in the atmosphere. Until Van Laer is able to bring forward some experimental evidence that this is not the case, his statement that his composite yeasts maintain their uniformity in composition will certainly be regarded with scepticism.

Finally, a question of considerable importance has reference to "the immunity as regards wild yeasts," which Van Laer claims for "beers fermented with certain symbiotic mixtures of Saccharomyces". He describes a comparative experiment in which wort was fermented in one case with an ordinary composite yeast, and in the other with a single race yeast, whilst the surrounding atmosphere contained "legions of wild ferments". The beer obtained with the ordinary yeast behaved in a normal manner, whilst that fermented with the single race yeast remained flat for about a fortnight and then "commenced to re-ferment with intense energy". In this experiment, the single race yeast employed was a variety which confessedly gave no after-fermentation and in consequence left the beer flat and in a

[1] *Trans. Inst. Brewing*, vol. vii., p. 95.

condition favourable for the development of the wild ferments. An experiment of this kind thus affords no argument against single race yeasts in general. At the same time, however, it cannot be denied that some varieties possess a greater power of resisting infection than others. This has in fact been shown by Hansen to be the case and Van Laer proves that the same holds good for composite yeasts.

In conclusion, it cannot yet be claimed that the question of the successful application of pure yeast in English breweries has been definitely solved. In view of the different character of the beers produced in different parts of the country, no general conclusions can be drawn until pure yeast has been tried in a considerable number of breweries. At the same time the success of single race yeast in a few breweries is undoubtedly of importance, proving its application for certain classes of beers, and this result should therefore encourage many brewers to give the Hansen system a fair trial.

A. K. Miller.

ELECTROSYNTHESIS.

IN the year 1800, Nicholson and Carlisle found that when a galvanic current was made to pass through water, this compound was decomposed into its elements, oxygen and hydrogen. The experiment excited a great amount of attention at the time, and numerous observations were at once made on the subject of electrolysis, or decomposition by means of the electric current. These culminated in the researches of Davy, who seven years later made his brilliant discovery of the alkali metals, sodium and potassium. On the theoretical side, the study of electrolysis was equally fruitful, for it led to the foundation of the electrochemical theory of Berzelius, which for more than a quarter of a century held the field to the practical exclusion of all others. According to this theory the bonds uniting the various components of chemical compounds were of electrical origin, and each compound could be resolved, in theory if not in practice, into an electropositive constituent and an electronegative constituent. These components again might each be resolved into electropositive and electronegative parts, until the atoms themselves were reached. The decomposition was in many cases effected practically by the electric current. Thus a solution of potassium sulphate, which was supposed to consist of electropositive potash K_2O, and electronegative sulphuric acid SO_3, is actually decomposed electrolytically into potash and sulphuric acid, which travel to the opposite poles of the battery. Potash again, when electrolysed, splits up into electropositive potassium and electronegative oxygen, the potassium going to the negative pole and the oxygen to the positive pole.

This mode of viewing chemical compounds was of great value in systematic chemistry, and possibly to it we owe the first example of electrosynthesis, although the true nature of the phenomenon was not at the time recognised.

Kolbe in 1849 subjected an aqueous solution of potas-

sium acetate to electrolysis, and found that methyl (ethane) was one of the products. Now, the formation of this substance was quite in accordance with the chemical theory of that day which regarded acetic acid as a paired compound of methyl and oxalic acid. Using the chemical symbols with their modern value, we should write Kolbe's acetic acid thus: $C_2H_6 \cdot C_2H_4O_2$. It was expected that when this was decomposed by electricity, the acid, which always plays the part of an electronegative constituent, would travel to the positive pole, and the methyl to the negative pole. But oxygen, due as was then supposed to the direct electrolytic decomposition of water, always appeared at the positive pole, so that the oxalic acid which was there liberated would be liable to oxidation; and since oxalic acid is a readily oxidisable substance it might be expected that its oxidation product, carbonic acid, would appear in its place, which was in fact found to be the case. But besides these gases, methyl, or as we now say, ethane, was also evolved at the same pole, in direct contradiction of the above theory of the constitution of acetic acid, according to which the ethane should have appeared at the opposite pole. From this pole only hydrogen was liberated. We shall presently see the modern way of explaining the reaction, but it is of some importance to understand the older mode of conceiving the process which went on at the positive pole, for it led to the erroneous notion that the formation of synthetical products there was due to oxidation, an idea which has not yet been wholly abandoned, although all recent work on the subject is at variance with it.

At the present day, and on the basis of the researches of Faraday and Hittorf, we conceive the phenomenon of the passage of an electric current through an electrolytic solution somewhat as follows. Whilst in a metallic conductor the electricity flows through matter, in an electrolytic conductor it flows with, and is carried by, matter. We imagine a continuous double procession of particles carrying electricity between the two poles or electrodes, the particles with negative electricity moving towards the positive

electrode, and the particles with positive electricity moving towards the negative electrode. These particles, or ions, are charged with definite amounts of electricity, all particles of the same kind having the same charge. For our present purpose we are not concerned whether the particles always exist in the solution in the electrically charged condition, or whether they merely assume that condition under the influence of the current: all that we need consider is that the ions arrive charged at the electrodes and there lose their charges. In a salt solution the metallic radical carries the positive electricity, and the acid radical the negative electricity. Thus in a solution of common salt, Na Cl, we have the positive ion, or cation, $\overset{+}{Na}$, and the negative ion, or anion, $\overline{Cl}$. As long as the ions retain their charges they must be looked upon as being different in many respects from the corresponding uncharged atoms or groups of atoms. When, however, they are discharged at the electrodes they regain their ordinary chemical characteristics and enter into the appropriate reactions. If they are capable of existing in the free state after discharge, they will be liberated as such; or they may react with each other, with the water in which they are dissolved, with the material of the electrode, etc. By suitable choice of conditions we may favour any of these possible modes of action at the expense of the others.

Let us now consider the case of potassium acetate investigated by Kolbe. The formula we ascribe to this compound is $CH_3 \cdot COOK$, and it will yield the ions $\overset{+}{K}$ and $\overline{CH_3 \cdot COO}$. The ion $\overset{+}{K}$ on discharge at a platinum electrode at once shows a characteristic property of potassium by reacting with the water of the solution, with formation of potassium hydroxide and liberation of hydrogen—

$$2K \cdot + 2H_2O = 2KOH + H_2.$$

When the anion is discharged at the positive electrode, also constructed of platinum in Kolbe's experiment, it can react either with the water, or with itself, according to the equations—

$$\text{I. } 2CH_3\cdot COO\cdot + H_2O = 2CH_3\cdot COOH + O$$
$$\text{II. } 2CH_3\cdot COO\cdot = CH_3\cdot CH_3 + 2CO_2.$$

In equation I. the products of the action are acetic acid and oxygen, in equation II. they are ethane and carbon dioxide, as Kolbe found.

It is evident that if we wish to favour the second action at the expense of the first, we must so arrange the conditions that the ions when they are discharged are as closely packed as possible in order that they may have an opportunity of acting on each other instead of on the water of the solution. This is best effected by having a concentrated solution and a small anode. In a strong solution there are a great many carriers of the electricity and when these arrive and are discharged at a comparatively small surface, they apparently react together in preference to acting on the water molecules in their neighbourhood.

Kolbe found that other products besides ethane, oxygen, and carbon dioxide appeared at the anode. The chief of these, indeed the only one whose existence has been confirmed by other observers, is methyl acetate. Its formation can easily be explained through the equation—

$$\text{III. } 2CH_3\cdot COO\cdot = CH_3\cdot COO\cdot CH_3 + CO_2.$$

Here again two discharged ions react, but now, however, they only lose one molecule of carbon dioxide instead of two as before. This product is never met with in considerable quantities.

There is still another possibility of reaction between two discharged ions, which, from the nature of the case, is only encountered when we are dealing with acids containing a greater number of carbon atoms in the molecule than acetic acid. Thus from the potassium salt of isovaleric acid $(CH_3)_2CH\cdot CH_2\cdot COOK$ Kolbe obtained not only diisobutyl $(CH_3)_2CH\cdot CH_2\cdot CH_2\cdot CH(CH_3)_2$ (eq. II.) but also large quantities of an unsaturated hydrocarbon $(CH_3)_2 CH : CH_2$, the formation of which is expressed by the equation—

$$\text{IV. } 2\ (CH_3)_2\ CH\cdot CH_2\cdot COO\cdot = (CH_3)_2\ CH\cdot CH_2\cdot COOH + (CH_3)_2CH : CH_2 + CO_2.$$

Beyond the two hydrocarbons, ethane and diisobutyl, synthetised by Kolbe, only two, *viz.*, butane and diisoamyl, have been made since his time by electrosynthesis, and it is only quite lately that the method has received any further extension.

In the case of dibasic acids, no synthetic action has been found to take place, the chief product being always an unsaturated hydrocarbon. For example, from potassium succinate, ethylene is obtained, the equation for the decomposition at the anode being as follows :—

$$\begin{matrix} CH_2 \cdot COO^{\cdot} \\ \dot{C}H_2 \cdot COO^{\cdot} \end{matrix} = \begin{matrix} CH_2 \\ \ddot{C}H_2 \end{matrix} + 2CO_2$$

When, however, instead of the dipotassium salt of a dibasic acid, the potassium ethyl salt is taken, one of the carboxyl groups becomes electrolytically inactive, and the salt behaves virtually as the salt of a monobasic acid. Thus Crum Brown and Walker found that potassium ethyl succinate yielded on electrolysis a synthetic product which proved to be diethyl adipate. The action is represented by the equation—

$$2\ COOEt \cdot (CH_2)_2 \cdot COO^{\cdot} = COOEt \cdot (CH_2)_4 \cdot COOEt + 2CO_2,$$

which is quite analogous to equation II. Now diethyl adipate can easily be converted into potassium ethyl adipate, and the electrolysis repeated ; this gives us a new synthetic product, namely, diethyl sebate in the same series of acids, thus—

$$2\ COOEt \cdot (CH_2)_4 \cdot COO^{\cdot} = COOEt \cdot (CH_2)_8 \cdot COOEt + 2\ CO_2,$$

so that here we have a means of passing synthetically from one acid to a higher member of the same series, and can therefore ascend the series until the method breaks down, a limit which has not yet been reached.

The following table shows the acids which have been prepared electrosynthetically in this way. It will be observed that in the synthesis the group of atoms between the terminal carboxyl groups (COOH) is simply doubled, the electrosynthetic products being always symmetrical.

Acid Corresponding to Potassium Ethyl Salt Electrolysed.		Acid Obtained by Electrosynthesis.	
Malonic	$COOH\cdot(CH_2)\cdot COOH$	Succinic	$COOH\cdot(CH_2)_2\cdot COOH$
Succinic	$COOH\cdot(CH_2)_2\cdot COOH$	Adipic	$COOH\cdot(CH_2)_4\cdot COOH$
Glutaric	$COOH\cdot(CH_2)_3\cdot COOH$	Suberic	$COOH\cdot(CH_2)_6\cdot COOH$
Adipic	$COOH\cdot(CH_2)_4\cdot COOH$	Sebacic	$COOH\cdot(CH_2)_8\cdot COOH$
Suberic	$COOH\cdot(CH_2)_6\cdot COOH$	Dicarbododecanic	$COOH\cdot(CH_2)_{12}\cdot COOH$
Sebacic	$COOH\cdot(CH_2)_8\cdot COOH$	Dicarbodecahexanic	$COOH\cdot(CH_2)_{16}\cdot COOH$
Methylmalonic	$COOH\cdot(CHMe)\cdot COOH$	Dimethylsuccinic[1]	$COOH\cdot(CHMe)_2\cdot COOH$
Dimethylmalonic	$COOH\cdot(CMe_2)\cdot COOH$	Tetramethylsuccinic	$COOH\cdot(CMe_2)_2\cdot COOH$
Ethylmalonic	$COOH\cdot(CHEt)\cdot COOH$	Diethylsuccinic[1]	$COOH\cdot(CHEt)_2\cdot COOH$
Camphoric	$COOH\cdot(C_8H_{14})\cdot COOH$	Camphothetic	$COOH\cdot(C_8H_{14})_2\cdot COOH$

[1] Two isomerides of the symmetrical dialkylsuccinic acids were obtained in each case.

The diethyl salts of the acids tabulated are not the only products of the electrolysis. Besides them there are formed the ethyl salts of monobasic unsaturated acids by a reaction analogous to that expressed by means of equation IV. The formation of these unsaturated ethereal salts always takes place to some extent, but is especially noticeable in the case of the acids with lateral branches, where the quantity produced is quite considerable. In the case of dimethylmalonic acid, for example, we have the ethyl salt of methylacrylic acid $CH_2:CMe\cdot COOEt$ formed according to the equation—

$$2CH_3\cdot\dot{C}Me\cdot COOEt = CH_3\dot{C}Me\cdot COOEt + CH_2:CMe\cdot COOEt + CO_2.$$

Whether the formation of complex ethereal salts, by an action of the type indicated by equation III., takes place, is a question which has not yet been investigated.

It is not every potassium ethyl salt of a dibasic acid that will yield an ethereal product on electrolysis. In fact, all the experience hitherto acquired seems to indicate that only saturated unsubstituted acids of the fatty series (including saturated rings) are capable of resisting the oxidising action at the anode, *cf.* equation I., sufficiently to permit the formation of ethereal compounds. Hydroxy-

acids, ethoxy-acids, chloro-acids, bromo-acids, unsaturated acids, and aromatic acids have all been tried, but instead of yielding synthetic products, or unsaturated compounds formed in accordance with equation IV., they only yield oxidation products, of a nature depending on the chemical characteristics of the acid electrolysed. The removal of hydrogen from a compound, or the introduction in its stead of chlorine, bromine, or a partially oxidised radical, seems to offer a point of attack for the active oxygen, which, even in the most favourable circumstances, is always produced at the anode, the whole molecule being then destroyed with formation of carbon monoxide, carbon dioxide, aldehydes, unsaturated hydrocarbons, etc.

The effect of various external conditions on the reactions taking place at the anode has been investigated by T. S. Murray, who subjected the electrolysis of potassium acetate solutions to an exhaustive examination. He found that the concentration of the solution and the size of the anode exercised an influence on the electrosynthesis precisely in accordance with the view expressed on p. 304. With a solution containing 0·5 per cent. of potassium acetate, the ethane produced was only equal to 0·3 per cent. of that theoretically obtainable from the current; with a 2 per cent. solution, the yield had increased to over 50 per cent., and with a 60 per cent. solution, it reached 80 per cent. of the theoretical amount. The effect of current strength was similar but not so marked. A current of 0·045 ampères gave a yield of 25 per cent., a current of 0·6 ampères, 69 per cent., and a current of 5·2 ampères, 81 per cent. The size of the anode is of great importance in determining the quantity of synthetic products formed. With a current of 2 ampères, and the surface of the anode 100 times greater than the surface of the cathode, the yield of ethane was 27 per cent. of the theoretical amount: with the same current but the electrodes reversed, so that the former anode became the cathode, and *vice versâ*, the yield rose to 88 per cent. The highest yield obtained in any case was 91 per cent. The temperature of the solution greatly affects the action at the anode. Thus, other conditions being the

same, the yield of ethane, which at 0° was 83 per cent., sank to 57 per cent. at 65°, and 12 per cent. at 95°.

Murray also investigated the amount of oxygen liberated under different conditions, and this placed him in a position to calculate the extent of oxidation occurring in each case. He arrived at the result that under the conditions which, according to the oxidation theory of the synthesis at the anode, would be most favourable for complete oxidation to take place, there occurred precisely the least amount of complete oxidation. The oxidation theory, too, is quite incompetent to account for the formation of methyl acetate from the acetic acid, and indeed the assumption that ethane itself is formed by the oxidation of acetic acid is one quite without an analogue in organic chemistry. The assumption on the other hand that the discharged ions interact directly, accounts for the formation not only of ethane, but of all the other products which have been observed.

Further confirmation of the latter view is to be had from the recent work of S. P. Mulliken, who electrolysed the sodium salts of compounds of the type of malonic ether. Malonic ether is scarcely to be regarded as a true acid, but it yields a compound $EtO{\cdot}CO{\cdot}CHNa{\cdot}CO{\cdot}OEt$ which is derived from it by the replacement of one of its hydrogen atoms by an atom of sodium. When the sodium atoms of two molecules are removed by an agent such as iodine, the residues unite with formation of the tetrethyl salt of ethanetetracarboxylic acid, thus—

$$\begin{array}{lll} \mathrm{COOEt} & \mathrm{COOEt} & \mathrm{COOEt\ COOEt} \\ \dot{\mathrm{C}}\mathrm{H} & \mathrm{Na} + \mathrm{I_2} + \mathrm{Na}\dot{\mathrm{C}}\mathrm{H} & = \mathrm{CH}\text{—}\ \dot{\mathrm{C}}\mathrm{H} + 2\ \mathrm{NaI} \\ \dot{\mathrm{C}}\mathrm{OOEt} & \dot{\mathrm{C}}\mathrm{OOEt} & \dot{\mathrm{C}}\mathrm{OOEt}\ \dot{\mathrm{C}}\mathrm{OOEt} \end{array}$$

Now the ions formed by the sodium salt are—

$$\overset{+}{\mathrm{N}}\mathrm{a} \text{ and } \begin{array}{l} \overset{-}{\mathrm{C}}\mathrm{OOEt} \\ \dot{\mathrm{C}}\mathrm{H}\cdot \\ \dot{\mathrm{C}}\mathrm{OOEt} \end{array} .$$

The negative ion on electrolysis of the solution will be

discharged at the anode, leaving an uncharged residue which combines with a similar one from a neighbouring ion, thus—

$$\begin{array}{cccc} \text{COOEt} & \text{COOEt} & \text{COOEt} & \text{COOEt} \\ \dot{\text{C}}\text{H}\cdot & +\cdot\dot{\text{C}}\text{H} & = \dot{\text{C}}\text{H} & \text{—} \quad \dot{\text{C}}\text{H} \\ \dot{\text{C}}\text{OOEt} & \dot{\text{C}}\text{OOEt} & \dot{\text{C}}\text{OOEt} & \dot{\text{C}}\text{OOEt} \end{array}$$

precisely as in the iodine synthesis.

As the sodium compounds of malonic ether and similar bodies are decomposed by water, it is necessary to conduct the electrolysis in alcoholic solution. The resistance of such a solution is very high, so that the current used by Mulliken did not exceed 0·3 ampères, the rate of production of the synthetic product being therefore in this case very slow.

In the aqueous solutions employed by Crum Brown and Walker, the current may be easily allowed to rise to 5 ampères in an ordinary platinum crucible if adequate cooling arrangements are provided, so that several hundred grams of ethereal products may be obtained even with a single apparatus in the course of a day or two's work. The yield is very satisfactory, reaching in ordinary practice to over half that theoretically possible. This method of electrosynthesis is by no means yet exhausted and will doubtless yield interesting results in the future.

Thus far we have used the term synthesis in the sense usually adopted by organic chemistry, namely, to express the linking together of carbon atoms which were originally separate from each other. In a more extended sense the term may be employed to denote the linking together of any two similar atoms. Thus corresponding with the synthetic action of iodine on the sodium salt of a carbon compound, alluded to on p. 308, we have a precisely analogous action of iodine on the sodium salt of a sulphur acid, two sulphur atoms being thereby linked together which originally belonged to different molecules. Sodium thiosulphate $\text{NaO}\cdot\text{SO}_2\cdot\text{SNa}$, is acted upon by iodine with

formation of sodium tetrathionate and sodium iodide, the action being expressed by the equation—

$$NaO\cdot SO_2\cdot S\,Na + I_2 + Na\,S\cdot SO_2\cdot ONa =$$
$$NaO\cdot SO_2\cdot S\cdot S\cdot SO_2\cdot ONa + 2\,NaI.$$

An apparently very similar synthesis by means of electrolysis has been performed by H. Marshall, who found that a concentrated aqueous solution of potassium hydrogen sulphate $KHSO_4$ when electrolysed yielded at the anode a new compound, potassium persulphate $K_2S_2O_8$. In all probability the ions in a strong solution of potassium hydrogen sulphate are $\overset{+}{K}$ and $HO\cdot\overline{SO}_2\cdot O\cdot$, so that at the anode we have on the discharge of the negative ion the following synthetic action :—

$$HO\cdot SO_2\cdot O\cdot + \cdot O\cdot SO_2\cdot OH = HO\cdot SO_2\cdot O\cdot O\cdot SO_2\cdot OH.$$

The persulphuric acid thus formed falls out of the solution as potassium persulphate, which is only slightly soluble under the conditions of experiment. We cannot state with certainty that the scheme given above represents the process of formation of persulphuric acid, as we are as yet too little familiar with the reaction, but in any event the electric current has here brought into existence a more complex compound from a simpler one, and we are thus entitled to regard the case as an interesting and novel electrosynthesis.

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JAMES WALKER.

GLYCOGEN.

IT is nearly fifty years ago that Claude Bernard discovered glycogen, or animal starch, and of all his epoch-making researches there is probably none which has been of such far-reaching importance, or the foundation of so many subsequent investigations. The distribution of glycogen in the liver, muscles and fœtal structures; the methods of obtaining it from those tissues; the chemical and physical characters of the purified material; all these points have been and still are to be found described and discussed in current physiological literature. No year passes but what something is added to our knowledge of glycogen, but even now the fundamental questions that go to the root of the whole matter are still subjects for dispute. These fundamental questions are two in number, namely: From what is glycogen formed? and what is the ultimate fate of glycogen?

I propose in the following article to describe certain recent work on the subject; this has more particularly concerned itself with answering the second of the two questions. To trace all the vicissitudes through which the theories of the fate of the hepatic glycogen have passed would be a tedious and thankless task; the general tendency of late years, however, has been to return once more to Claude Bernard's original teaching that glycogen is what its name professes it to be, the mother substance of sugar.

Before, however, passing to the consideration of the fate of the hepatic glycogen, it will be convenient first to state the case with regard to its origin. Bernard considered that proteid food was the source of glycogen in the liver; Pavy, on the other hand, supposed that it was from carbohydrate food alone that glycogen was derived. As is usually the case in physiological investigations where there are two opposite theories, the truth is ultimately found to lie somewhere between them, and the view usually accepted at the present day is that both varieties of food may act as sources

of glycogen. The glycogen is most abundant after carbohydrate food; but it also occurs in animals kept on a proteid diet. In the latter case it must originate by complex intra-protoplasmic or metabolic changes. In the former case it is probably a storage of reserve carbohydrate, and the chemical processes involved are simpler. C. Voit and his pupils (1) have taken up this subject and by experiments on rabbits and hens investigated what forms of carbohydrate may be stored up in the liver as glycogen. The hepatic glycogen was first reduced to a minimum by a few days' inanition. Large doses of different sugars were then given, the animal killed and the estimation of the glycogen performed either by Brücke's or Külz's method.

Dextrose, levulose, cane sugar and maltose increased the glycogen; lactose and galactose did not, or only very slightly. In the alimentary canal cane sugar is inverted, equal parts of dextrose and levulose being formed from it. Maltose undergoes a corresponding hydrolytic change, two molecules of dextrose being formed from it. The liver, in other words, forms glycogen from those sugars which are converted into dextrose and levulose before they reach it. In fact, these appear to be the only forms of sugar which when present in the blood lead to a storage of glycogen in the liver; when subcutaneously injected the hepatic glycogen rises in amount. No other form of sugar behaves in this way. In regard to levulose (a ketone) a point still unsettled is whether the liver cells form glycogen from it direct, or after previously changing it into the corresponding aldehyde, dextrose. E. Fischer's researches would indicate that the latter possibility is not improbable; moreover the action of the cells of the mammary gland in converting dextrose into lactose is somewhat analogous.

Questions of this kind derive practical importance because the subject of glycogen forms the starting-point for the consideration of the pathological condition called diabetes, and F. Voit (2) carried on the research on a diabetic patient, who was taking restricted diet, and the amount of sugar in whose urine was thus reduced to a low ebb. On one occasion 100 grammes, on another 150 grammes of

milk sugar were administered; the sugar in the urine at once went up. A pure culture of *saccharomyces apiculatus* was added to the urine, and fermentation allowed to go on. After a few days' fermentation the sugar had all disappeared. This fungus causes the alcoholic fermentation of glucose, but not of lactose. Thus there was no lactose in the urine. Voit gives the following explanation of what had occurred: "Before the sugar was given the small amount of diabetic sugar in the urine must have originated from proteid metabolism. Lactose appears in the urine more readily than any other form of sugar, though the doses given in this case were not large enough to cause this result. Lactose readily appears in the urine, not because it is burnt in the organism with great difficulty, but because it is a form of sugar from which it is not possible for the liver to make glycogen; therefore its 'assimilation limit' is low and it passes unused into the urine. In fact, in diabetics it appears to be more readily burnt in the body than dextrose and takes the place of some of the dextrose originating from proteid metabolism. Hence, when lactose is given, a large quantity of dextrose remains unburnt and passes as such into the urine." A further research (3) carried out with galactose gave similar results.

But we must now return to the other fundamental question, namely, that concerning the fate of the hepatic glycogen; and in connection with this subject two important papers have recently appeared. One is by Dr. D. Noël Paton and the other by M. Bial.

"L'acte *vital* c'est la production du glycogène au sein du tissu vivant."

"L'acte *chimique*, c'est la transformation du glycogène en sucre."

These two propositions give in Bernard's own words his views in a nutshell; and it is the second with which Dr. Paton (4) is concerned, his object being to elucidate the meaning of the expression *chimique*. Is the conversion due to an enzyme or unorganised ferment as held by Bernard, or to the vital action of the liver protoplasm as suggested by recent writers? The liver was roughly minced and kept at 37° to 40°C. in normal salt solution; the glycogen was then

estimated by Brücke's method. It was found that in the first half-hour the disappearance of glycogen was very rapid; it then slowly and steadily diminished for a few hours afterwards.

If, however, the liver substance is destroyed by grinding it up with sand, the loss of glycogen is inconsiderable when compared with control specimens, in which the liver was simply roughly minced and in which the cells would therefore retain their integrity.

Microscopic examination of the liver cells in salt solution shows that they undergo certain degenerative changes which usually begin within the first hour.

The conversion of glycogen is, therefore, divided into two periods.

(1) An early period of rapid conversion occurring before obvious structural changes occur in the liver cells.

(2) A late period of slow conversion after these changes have developed.

The first rapid change is inhibited by destroying the liver cells by a temperature of over 60° and by one per cent. solutions of sodium fluoride. It is regarded as being produced by the action of the living liver cells. The second slower change is apparently not affected by these agencies, and is regarded as being due to the activity of an enzyme.

Chloroform was found to increase hepatic amylolysis, and to accelerate the structural changes in the liver cells; it acts probably by stimulating the katabolic changes in the cells which precede their death. Chloroform would appear to act in the same way during life; glycosuria is by no means an infrequent accompaniment of chloroform narcosis, and in pairs of animals of the same litter, one chloroformed, the other not, the hepatic glycogen was always found to be more abundant in the latter case. Ether acts in the same way as chloroform, only to a less degree. Pyrogallic acid (0·25 per cent. solution) acts similarly. Morphine, curare, amyl nitrite and sodium salicylate have no action.

The products of hepatic amylolysis were also investigated. In the early stage glucose appears to be formed directly, and no intermediate substances, such as dextrins

or maltose, occur. In the later stage, the former of these, possibly the latter, are always found.

The early rapid amylolysis is the result of the katabolic changes in the protoplasm accentuated as death occurs; it is, in fact, simply an exaggeration of the process of amylolysis during life. The later slow amylolysis is due to an enzyme which is probably developed during the disintegration of the liver cells. It is not due to acid, though lactic acid is formed, and the action of micro-organisms is excluded by another set of experiments.

Passing now to the experiments of M. Bial (5) we find that the question he attempts to answer is the same as that which Dr. Paton tried to solve. The plan of the research is, however, different, and his final conclusions are not quite the same. So after giving a brief summary of his paper it will be necessary to compare the two researches and try if it is possible to reconcile the differences.

Put briefly, the object of Bial's paper is to show that Claude Bernard's second proposition is absolutely true; the sugar is formed from glycogen by the action of a diastatic ferment. This diastatic ferment of the liver is probably identical with the diastatic ferment of the blood; at any rate both form dextrose and not maltose from starch.

The most prominent opponent of Bernard's views on this subject in former years was Dr. Pavy. Dr. Pavy was unable to find more sugar in the blood of the hepatic vein than in that of the portal. He therefore concluded that the formation of sugar *post mortem* was different from what occurs during life. This view of Pavy's, resting on a narrow vitalistic basis, has been also upset by the discovery of better methods of estimating sugar in blood. Bial, therefore, reserves his powder and shot for a more recent opponent of Bernard's views, namely, Seegen. Seegen (6) preaches the revolutionary doctrine that the sugar which leaves the liver is not formed from glycogen at all, but from peptone, or even from fat. Seegen has made very few converts: his method of experiment and of reasoning is such that any one accustomed to chemico-physiological investigations can easily detect his fallacies. Besides this,

more than one worker has already demonstrated the inaccuracies of his experimental premisses. Neumeister (7), for instance, puts the case as follows:—

First, he shows that the liver cannot make sugar from peptone, as no peptone ever reaches the liver under normal circumstances; it is reconverted into albumin during its passage through the intestinal epithelium. Feeding experiments with peptone do not therefore prove anything.

Secondly, injection of peptone into the portal vein is also fallacious; the injection of this toxic material leads to such grave changes in the circulation that any increase of sugar discharged from the liver would more probably be derived from the increased decomposition of glycogen than from the substance injected.

Thirdly, Seegen finds that pieces of liver removed from the body and digested with blood and peptone yield more sugar than other pieces of the same liver which are not so treated. Neumeister points out that the method used for the estimation of sugar under those circumstances is fallacious; the sugar was estimated by Fehling's solution in the presence of the peptone; some of the copper would be taken up in causing the biuret reaction with peptone, so that the remainder would only require comparatively little sugar to reduce it, and if this factor was neglected one would conclude erroneously, as Seegen did, that the percentage of sugar had risen in the fluid investigated. In the liver of a fasting animal containing no glycogen digestion with peptone and blood causes no appearance of sugar.

Bial's own experiments may be briefly summarised as follows:—

1. Digestion of liver with blood leads to a greater formation of sugar than digestion of liver with salt solution. This is due to the fact that blood contains a diastatic ferment (8).

2. Peptone, like saliva or malt extract, favours the activity of this ferment on solutions of glycogen or starch paste, but leads to the formation of no more sugar from pieces of liver than in those specimens to which blood alone had been added.

3. Digestion of blood with peptone never leads to a formation of sugar. This disposes of Lépine's (9) pepto-saccharifying ferment.

4. Arthus and Huber (10) have shown that sodium fluoride in one per cent. solutions destroys cellular activity but not unorganised ferments or enzymes. The formation of sugar from glycogen is not prevented by a one per cent. solution of sodium fluoride, and, therefore, it is produced by an unorganised ferment. This is produced in the liver cells, though whether the liver cells derive it from the blood and lymph which bathe them is still unsettled. Certainly the liver ferment is very similar to that found in the blood. Both produce dextrose, not maltose, as their end-product.

Now, if we compare these results with Noël Paton's we find that they agree in regarding sugar as the result of the disappearance of glycogen, that the sugar formed is dextrose, and that an enzyme is present which brings about the conversion of glycogen into dextrose. The main difference between the two observers is that Paton considers the vital action of the liver cells as more important than the chemical action of the ferment, while Bial supposes that the whole conversion is accomplished by the ferment. The reason for this difference appears to be that Bial, having found the ferment, concluded that he need seek no farther. If he had he would doubtless have discovered that the vital action of the cells is to be reckoned with as well.

The term "vital action" is one employed pretty largely by physiologists now-a-days. It has not the mystic significance it had in the days of the old vitalists from Stahl to John Hunter. But it is a convenient term for those metabolic or selective processes in living cells which cannot be at present explained by ordinary chemical or physical laws. A ferment is a chemical substance shed out by a living cell, and the change produced by a ferment is usually a hydration or hydrolysis. Thus we have the formation of maltose from starch by saliva, the formation of peptone from albumin, the fat-splitting action of the pancreatic juice, the inversion of cane sugar, and many other examples that might

be given. "Vital action," as distinguished from ferment action, is generally a dehydration; as examples one may take the formation of albumin from peptone by the intestinal epithelium, or the formation of glycogen from sugar by the liver cells. In the present state of our knowledge, or rather of our ignorance, it is unwise, however, to limit the term "vital action" to processes of dehydration and to assume that a hydration must of necessity be produced by a ferment, and that the metabolic activity of cells cannot bring about hydration as well as dehydration without the intermediation of an enzyme.

If we assume with Paton, Ransom and others that in the liver cells vital action will explain the origin of sugar from glycogen, the question next arises whether it is possible to give any other example where living cells produce a hydrating action. There is one other example which appears to be very similar, and that is the formation of glucose from maltose in the intestine. The experiments of Dr. Shore and Miss Tebb (11) indicate that this change cannot be wholly explained as due to the activity of the inverting ferment of the intestinal juice, but that the vital action of the absorptive epithelium forms a factor in the phenomenon also.

We have seen that the ordinary source of the liver glycogen is the carbohydrate of the diet; but that it may originate from proteid under certain circumstances. We have seen also that the ordinary source of the sugar of the blood is the glycogen of the liver; it is necessary in conclusion to point out that sugar may also originate from the proteid of the liver cells; and here it must certainly be produced by metabolic or vital activity. There are certain forms of diabetes which are entirely uninfluenced by diet; the patient passes as much sugar when he takes no carbohydrate as when he is on starchy food; and this form of diabetes can be produced artificially. If the drug phloridzin is given to an animal it becomes intensely diabetic. The sugar might under these circumstances have a threefold origin. It might come from the drug given, for phloridzin is a glucoside. But the sugar passed is far in excess of that

contained in the drug; and phloretin, a sugar-free derivative of phloridzin, produces diabetes just in the same way. Secondly, it might originate from the hepatic glycogen, but this cannot be the only source, as the drug produces diabetes in fasting animals, and also in those who have been taking it for some days and in which all the glycogen must have been got rid of long ago. Thus the third possible source is alone left, and the sugar undoubtedly comes from proteid.

But how can proteid give rise to sugar? Is it possible to obtain sugar from proteid in the laboratory? This is a question which it is not possible to answer fully in the present article. Suffice it to state that recent research has shown that the answer to this question will probably be found to be an affirmative one. In no case as yet has dextrose been obtained from proteid; but some recent experiments (12, 13, 14) teach us that sugars other than dextrose are obtainable, and that is a step in advance.

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W. D. HALLIBURTON.

MESOZOIC AND KAINOZOIC GEOLOGY IN EUROPE.

IT would be vain to attempt, within the space of a single article, to give even the barest outline of the present state of stratigraphical geology or of any considerable section of that science. All that can be done here is to chronicle the advances which are made from time to time in its various branches. Fortunately, however, for the purpose in view, the progress of our knowledge of the great geological systems is not uniform. Sometimes there appear almost simultaneously a number of important observations on the rocks of one system while another perhaps is apparently neglected; and at other times the cases may be reversed. Thus within the last few months several valuable papers have been published on Miocene geology; but few have appeared which refer to the Trias. Hence the present article will to a certain extent afford a summary of the state of Miocene geology in Central Europe; while it may be hoped that some future article will perform the same service for the Trias. It is only in this piecemeal fashion that we can expect to sketch the present state of stratigraphical geology and at the same time to keep pace with its progress.

Considered by itself, the geology of a single area is of little interest except to those who have worked or lived in that area; and it is only when we attempt to compare the local geology with that of other parts of the world, that its full value can be weighed and determined. In England perhaps we were somewhat slower to perceive this than our colleagues abroad; and it was natural that this should be so. For the science of stratigraphical geology took its rise in England, and it was here that nearly all the greater geological divisions were first recognised and described. It was necessary, therefore, for foreign geologists to compare their rocks with ours; but it did not seem so clear to us that ours must also be compared with theirs. Moreover,

from a geological point of view England really serves as a table of contents to the greater part of the Northern Hemisphere, and it does, better than any other country, afford a general idea of geology as a whole. But, however admirable a table of contents may be, the study of that table can scarcely replace the study of the whole book.

In these articles the treatment will be designed, as far as possible, to bring out the mutual geological relations of different areas; and for this purpose the arrangement will be according to the stratigraphical systems. But a section will be devoted to papers of a general character, and this section will include a certain number of local descriptions which do not find a convenient place elsewhere.

At the end of this article will be found a list of the more important papers which have been published (or have been received in England) since the beginning of last October, and it is to these that the present notice refers. Where it has been necessary to mention an earlier paper the reference will generally be found in a footnote.

GENERAL.

Among the many debts which modern geology owes primarily to Heim, and in Britain to Lapworth, one of the greatest has been the recognition of the fact that a whole mountain mass may be slid over the top of other rocks for a distance of several miles. Since the researches of Peach and Horne[1] in the North-west Highlands the sliding surface has been known as a thrust-plane; and it has been found that the existence of thrust-planes removes many difficulties which had been felt in the interpretation of other disturbed areas.

In the neighbourhood of Toulon, for example, which has recently been described by Zürcher (42), the relations of the schists and crystalline rocks to the beds of later age were incapable of explanation by means of simple inversions and ordinary faults. Zürcher shows that if we suppose the

[1] *Nature*, xxxi. (1884), p. 29. *Quart. Journ. Geol. Soc.*, xliv. (1888), p. 378.

schists to have been brought upon the top of the newer beds by means of a thrust-plane, the whole difficulty disappears. A thrust-plane, however, like any other divisional plane in a mass of rock, must be affected by any foldings which may take place subsequently in that mass; and in the Toulon area the gliding surface is not now a true plane, in the mathematical sense of the word, but has been thrown into a series of anticlinals and synclinals which run nearly east-west.

A much more striking instance of a thrust-plane, the evidence of which, however, is not so clear, has been brought forward by Schardt (32). The northern zone of the Alps consists of ranges of hills which lie in front of the main chain and which are often spoken of as the Limestone Alps or Pre-Alps. Geographically this zone is continuous, but geologically the portion which extends from the Chablais (south of Lake Geneva) to the Stockhorn (west of Lake Thun) is quite distinct from the south-west and north-east parts of the zone, which are known respectively as the Faucigny and Unterwald. The folds of the Alps of the Faucigny and Unterwald are not the continuations of those of the Chablais and Stockhorn, and the thrust-planes which are present in the latter[1] are unknown in the former. Moreover the character of the rocks is different, and Schardt looks upon the Pre-Alps between the Chablais and the Stockhorn as a mass of foreign rock which has been brought from the south upon a gigantic thrust-plane. He places the original position of this mass in line with the Rhaetikon (on the Austrian border), and considers it probable that the thrust-plane may be a continuation of that of the Rhaetikon.

Lugeon (22) in a somewhat earlier paper gives a different explanation of the structure of the Chablais. He supposes that the beds of that area were thrown into a dome, which bulged out at the sides and was pinched in at the base, so that all around the edge the beds were reversed

[1] In another paper (33) Schardt gives sections of the Pre-Alps near Montreux on the northern shore of Lake Geneva.

and came to rest upon newer rocks. Startling though M. Schardt's view may appear, however, it harmonises better with the structure of the surrounding areas.

According to Bertrand (4) the French Alps present the fan-structure which is so frequently found in mountain chains. The centre of the fan is formed in Savoy by a band of coal measures which runs nearly north-south. To the east of this almost all the folds lean towards Italy, and to the west they lean towards France. Further south the central zone is occupied not by the oldest, but by the newest rocks (Nummulitic and Flysch). In places the lines of folding, which in general follow the direction of the chain, open out around lenticular masses or kernels of rock. The rocks of these masses are themselves thrown into folds of the same direction, but these folds are not prolonged beyond the edges of the lenticles. Mont Blanc itself is one of these kernels.

Turning from great things to small some remarkable faults have been described by Karrer (18) in tertiary beds in the Vienna basin. In some of the sections which he gives, the upper beds are disturbed by a series of small faults which terminate abruptly against the beds below.

We must here notice an account given by Dames (9) of the geology of Heligoland. The rocks of this island have usually been referred to the Triassic, Jurassic, and Cretaceous systems; but according to Dames the Jurassic is absent, while on the other hand the Permian also is represented.

Even at a distance the main island can be seen to be made up of two series. The lower consists chiefly of reddish-brown thick-bedded calcareous clays in which copper ores occur; and from the presence of these ores and the close resemblance to certain rocks of Schleswig-Holstein, Dames refers these beds to the Zechstein (Permian). The upper series shows irregular alternations of red shaly clays with bands of sandstone and limestone, and without copper, and this he correlates with the lower Bunter.

The nearest islets to the east of the main island are Wite Klif, and Olde Höve Brunnen; and it was the rocks of

these which were referred by Wiebel to the Jurassic, but their fossils are typical upper Muschelkalk forms. Neither the lower nor middle Muschelkalk is now seen *in situ*, though their boulders are abundant; but at the beginning of last century, the latter, consisting of gypseous beds, formed a cliff parallel to the main island. The lower and upper Muschelkalk consist of clays and dolomitic limestones.

The dolomites of the Wite Klif are succeeded towards the east by clays and chalky beds, which occupy the floor of the Skit Gatt, but are now buried in sand. Zones of belemnites may be distinguished which correspond closely with those of the Speeton Clay of Yorkshire. Perhaps the most interesting is that of *Belemnites fusiformis*, which consists of red chalk. The whole of the lower Cretaceous appears to be represented except the middle Gault.

The rocks which lie to the east of the Skit Gatt and which are left dry at the ebb, consist of white chalk. This was formerly referred to the zone of *Belemnitella mucronata;* but really contains representatives of the Cenomanian, Turonian and Senonian.

TRIAS.

Passing now to the consideration of the geological systems in order, we commence with the Trias.

In a paper on coral in the dolomites (25), Miss Ogilvie returns to the subject of these rocks in Southern Tyrol. She has already[1] dealt in detail with the stratigraphy of some parts of this area, and her chief object here is to show that it is the "cipit" limestones, and not the great masses of dolomite, which deserve the name of coral reefs.

During early Triassic times the central core of the Alps was land; and thus the sea was shallow in the northern part of Southern Tyrol, and deeper towards the south. Somewhat to the north of the present Schlern Mountain there was a great eruption of volcanic material under the sea, and this formed a ridge or barrier some distance from the coast. At about the same period

[1] *Quart. Journ. Geol. Soc.*, vol. xlix. (1893), p. 1.

contemporaneous faulting took place along the line of eruption, and the floor of the sea outside this line was let down. Hence between the volcanic ridge and the land there was shallow water, and outside the ridge a deep sea. Within the former marls and shales were rapidly deposited; while in the latter the dolomites and limestones were laid down, and they owe their great and suddenly increasing thickness to the subsidence of the sea-floor. Corals grew within the shallow water and upon the volcanic ridge, forming the lenticular masses which are known as "cipit limestones," but the dolomites themselves appear to have been formed by algæ and mollusca.

At a later period the subsidence became general and the dolomites spread over the barrier into the former shallow water area; while towards the close of the Triassic period, the sea again became shallower, and shallow water deposits spread outwards from the north towards the south.

Much of the difficulty which has been felt in the examination of the Tyrolese Trias has been due to the fact that deposition was going on simultaneously under very different conditions in different areas. And it is worthy of note that the same difficulty has been experienced in Southern Italy. Near Lagonegro, in the province of Basilicata, there are developed nodular limestones, silicious schists with radiolaria, and dolomitic limestones. The mutual relations of these beds are not clear; but Lorenzo (20) has come to the conclusion that they are simply heteropic developments of the same period. From an examination of the fossils, he places them about the horizon of the Cassian and Raibl beds of Tyrol.

In the Balearic Isles, according to Nolan (24), the Upper Trias is divisible into three distinct horizons,[1] which correspond very closely with those of Southern Tyrol; and these islands must therefore be placed in Mojssovics' Mediterranean Province.

[1] These are, in ascending order:—

1. Zone of *Trachyceras Curionii* and *T. Vilanovæ*.
2. Zone of *Daonella Lommeli*.
3. Zone of *Hoernesia pseudosocialis* and *Monotis salinaria*.

But little has been added to our knowledge of the Trias in Northern Europe. Stizenberger (35) has described the rocks exposed on the railway between Koblenz and Stein; but they offer no points of special interest.

JURASSIC.

The most important contributions to Jurassic geology come from our own country; and we must first draw attention to the monograph by H. B. Woodward (41) on the Lias of England and Wales (exclusive of Yorkshire, which has already been treated by Fox-Strangways[1]). This work is too extensive and too much occupied with local detail to admit of useful abstract here. It is more particularly concerning the south-western parts of England that new facts will be found in it. At the end is a list of fossils from the English Lias (exclusive of those which occur only in Yorkshire, north of the Humber); and it is worthy of note that the well-known *Gryphea incurva* appears as *G. arcuata*, a name which certainly has priority, and which is commonly used abroad.

In the district between Sherborne in Dorsetshire and Yeovil in Somersetshire S. S. Buckman (7) has made a detailed examination of those beds of the Inferior Oolite which are usually known as the zones of *Ammonites Murchisonæ*, *A. Sowerbyi*, *A. Humphriesianus*, and *A. Parkinsoni*. But the author is far from being content with this grouping, and he subdivides the period of deposition into twelve smaller divisions which he calls "hemeræ". By a *hemera* he understands the period of time during which a certain species was at its acme of development; and each of his twelve hemeræ is characterised by its own ammonite.

The area described is only some eight miles in length, and yet within this distance the deposits of the hemera of *A. Garantianæ*, for example, vary from less than a foot to thirty feet in thickness. Among the other deposits the variations are also very great, and the author shows them

[1] The Jurassic Rocks of Britain, vols i. and ii. *Mem. Geol. Surv.*

by means of a graphic diagram. During the earlier part of the period the area of maximum deposition moved from the centre of the district towards the east; while during the later hemeræ it moved towards the west.

It is quite possible, as Buckman points out, that two species which occur together in the same bed of rock may really not have been contemporaneous and that proof of the difference of age may be obtained elsewhere. At one place we may have a great accumulation of deposit, with a certain species at its base and another at its summit, while some distance off the same period of deposition may be represented by so thin a bed that the two species occur together. Yet the one species is newer than the other. In the western part of the Sherborne area, for example, the deposits of the various hemeræ are so thin that it is not possible to distinguish them all; but as the beds thicken towards the east, it becomes easy to separate those of the individual hemeræ, and the characteristic ammonites appear, not together but in their regular order.

Comparison with the rocks of Normandy, the south of France and Wurtemberg shows that the general succession of the faunas is the same; but it can scarcely be maintained as yet that all of Buckman's hemeræ have been distinguished in those areas.

A description of the uppermost Jurassic, or Purbeck beds, as they occur in the Vale of Wardour in Wiltshire, has been given by Andrews and Jukes-Browne (1). The whole series is remarkably like that of the coast of Dorsetshire and has been deposited under nearly the same conditions. Thus the lower Purbeck consists of fresh-water beds towards the base and towards the summit, while the presence of *Cardium*, *Corbula*, etc., in the middle portion shows that the waters were then brackish: the middle Purbeck includes a "cinder-bed," which corresponds with that of Dorsetshire and which is often crowded with *Trigonia* and *Ostrea*, showing the incoming of marine conditions. The upper Purbeck is more distinct in character and near its base includes a soft yellow sand with *Endogenites*, which appears to be peculiar to Wiltshire. Above, it consists mainly of clays and marls.

In France the two zones which M. Chalmas[1] recognised in the upper assise of the Lias of Normandy (*Grammoceras aalense* and *Lioceras opalinum*), but which he did not find in direct superposition, have been so found by Brasil (6) at several places around Caen. Here, as in Germany, the *aalense* zone is the lower.

Immediately to the north-west of the Jura Mountains the Abbé Bourgeat (5) has examined the Oxfordian and Corallian of the region between Dôle, Salins, and Besançon. The ferruginous banks with *Ammonites macrocephalus* and *A. anceps*, which occur in the Jura proper and which correspond with our Kellaways rock, are absent; and the Oxfordian rests immediately upon a limestone with *Rhynchonella varians*, evidently the equivalent of the upper part of our Great Oolite series. The Oxfordian itself consists of marls and limestones, with numerous sponges; and several of the beds as they are traced towards the Jura lose the littoral character which they possess in the west of the area. The succeeding Corallian consists of limestones with bryozoa; and was a shallow-water deposit. From this author's account one would be justified in concluding that during these two periods the land lay not far to the west, towards the Côte d'Or.

In the vast expanse of the North German plain it is seldom that the beds below the drift are exposed; and any addition to our knowledge of the rocky substratum is very welcome. Some forty years ago in the Island of Wollin at the mouth of the Oder, Wessel[2] found numerous loose blocks with fossils which he referred to the Oxfordian, and the same fossils occurred also in a clay which he discovered *in situ*. Deecke (10), in re-describing the area, points out that the fossils belong to what we now know as the Callovian, or to the Cornbrash (upper Dogger of the Germans). At another point a bed occurs *in situ* with *Belemnites giganteus*, which belongs to the middle Dogger. In this paper Deecke attempts a classification of the rocks

[1] *Bull. Soc. Geol., France*, vol. xx. (1892-93), p. clxiii.

[2] *Zeitschr. Deutsch. Geol. Ges.*, vi. (1854), p. 305.

of the various exposures of upper Dogger scattered throughout Pomerania.

Somewhat farther to the south, Fiebelkorn (12) has been engaged in the study of the upper Jurassic boulders which occur in the drift, and has published an elaborate examination of the included fossils. These boulders are not commonly met with and hitherto have not received so much attention as the boulders of other rocks. Their rarity appears to be due to the friable nature of the material, and to the fact that they occur chiefly in the lower boulder clay, which is naturally less exposed than the upper. They are found only in Pomerania, Brandenburg, and Mecklenburg, and are beyond comparison most abundant at Retzin, west of Stettin. Their abundance here, together with their friable nature, shows that the original home of these blocks cannot have been far from that place.

Turning now to the South of Europe we find that in the Balearic Islands the lower and middle portions of the Jurassic system closely resemble the corresponding beds of Northern Europe. According to Nolan (24) the middle Lias has a fauna which is nearly allied to that of Portugal, but quite different from that of Andalusia in the South of Spain; the upper Lias also resembles that of Portugal and Lombardy; while the Bajocian may be divided into zones which correspond precisely with those of the Anglo-Parisian area and of Swabia. The upper part of the Jurassic, however, belongs distinctly to the Mediterranean or Tithonian type, and is entirely different from that of Northern Europe.

Concerning this Tithonian facies as it is developed in the East of Europe, Retowski (28) has given an admirable account of the fauna from Theodosia in the east of the Crimea. The rocks consist mainly of yellowish or grey marls with numerous fossils. Of the sixty-five forms described nearly half are new, and of the remainder almost all occur in the upper Tithonian of Western Europe, while ten are peculiar to those upper beds. The presumption is that the Theodosia beds belong to the upper division of the Tithonian. This view is confirmed by the abundance of species of *Hoplites*: and by the fact that the beds are

immediately overlaid by those marls with *Belemnites latus* which in the South-west of Europe also overlie the Tithonian, and are there known as Valenginien. Tithonian beds also occur to the west of Theodosia, and the series appears to be well developed in the Crimea.

CRETACEOUS.

The occasional patches of Jurassic rocks which have already been referred to, in the North German plain, occur principally around the mouth of the Oder. Farther to the west, and stretching as far as the Elbe, there appear through the drift a number of small patches of Cretaceous. One of these, near Lüneberg on the Ilmenau, has long been supposed to include representatives of the Gault in the form of a clay with *Belemnites minimus*. The clay is conformably succeeded by a limestone which passes up into sandstone with *Ammonites rotomagensis* (upper Cenomanian). Von Strombeck (36) after a close examination concludes that the belemnite is *Belemnites ultimus*. It is associated with *Serpula* (*Vermicularia*) *Sowerbyi;* and both of these are Cenomanian forms. Hence he refers the whole series up to the *rotomagensis* beds to the Cenomanian, and places the clay at the base as the representative of the Tourtia or lower division. No Gault is present; and the author maintains that the reference to the Gault of other exposures in this neighbourhood rests upon similar incorrect determinations. This is a point of some importance; for it is certain that the Cenomanian sea was much more extensive than that of the Gault, and if there has been no Gault here, the sea of that period must have been even smaller than has been supposed.

To the west of Glatz on the borders of Bohemia and Prussian Silesia, a long elliptical patch of Cretaceous rocks rests unconformably upon the older rocks of the Sudetengebirge. It is now entirely separated from the Cretaceous of the North-east of Bohemia; but it evidently belonged to the same sea-basin, and between the two areas lie a number of small outliers bearing witness to their former union. One of the most extensive of these outliers is that of Cudowa, which has been examined in detail by Michael (23).

The rocks of this patch have hitherto been referred to the Cenomanian; but Michael shows that the upper portion must be separated off as Turonian. The Cenomanian commences with a conglomeratic and calcareous series which rests immediately upon the granite of Cudowa. This is succeeded by "Quader" and "Pläner" sandstones capped by a glauconitic bank. Throughout the whole the commonest fossils are *Exogyra columba, Ostrea carinata, Pecten asper* and *Janira longicauda;* but the upper beds also contain forms which are not elsewhere confined to the Cenomanian. It is clear, however, that the zones which Schluter has established for the Cenomanian of Westphalia cannot be distinguished here.

Above the glauconite bank come clayey and sandy beds, calcareous in the lower part, free from lime in the upper. These contain *Inoceramus labiatus, I. Brongniarti* and other fossils; and must clearly be referred to the Turonian.

Here, as in Saxony and many other parts of Europe, it will thus be seen that the Cretaceous begins with a Cenomanian conglomerate, showing that the Cretaceous sea first began to spread over these regions in Cenomanian times.

Passing over to Italy it has been shown by Sacco (31), in the northern part of the Apennines, that a considerable portion of the beds which have hitherto been referred to the Eocene under the name of "argiles écailleuses" and "schistes ophiolithiques" must really be of Cretaceous age. He describes the fossils, and amongst them we have such typically Cretaceous forms as *Hamites, Inoceramus Cripsi,* etc.

In the northern part of the province of Venice a band of Cretaceous rocks lies along the lower slope of the Alps from near Bassano to the valley of Tagliamento; and the north-east portion of this band has been described by Futterer (13). It is here bounded approximately by two great faults, both of which throw down towards the south. Between the Cretaceous and the older rocks of the mountains to the north, there is the great peri-adriatic fault; while to the south the Cretaceous band is separated from

the Miocene and later deposits of the plain by the nearly parallel fault of Aviano. Between these two faults the Cretaceous limestones are thrown into the form of a broad arch. On the northern flanks of the arch there usually rest some remains of the scaglia or even of the lower Tertiary; but on the southern flanks these beds are absent, and the Cretaceous limestones are in immediate contact with the fault.

In these limestones three distinct horizons can be recognised: the upper beds are characterised by the abundance of *Radiolites;* the middle by *Hippurites* (*H. cornuvaccinum*, etc.); and the lower by Caprinidæ. No definite lithological or paleontological horizons can be recognised below the last.

The same author has described (14) the hippurites from beds of corresponding age at Nabresina, on the coast north-west of Trieste.

TERTIARY.

A considerable portion of the valley of the Rhone is occupied by Miocene deposits, which lie between the Alps on the east and the Central Plateau of France upon the west, and which extend more or less continuously from the coast of Provence to Savoy. It is clear that there was here in Miocene times a great arm of the sea, which during certain epochs appears to have reached the plain of Central Switzerland. But the conditions were not uniform during the whole of the period, and only the middle portion of the Miocene is marine throughout the whole area.

According to Depéret (11) the Tertiary deposits of the Rhone basin may be grouped as follows (in ascending order):—

Upper Oligocene.

I. *Aquitanian.*—Rests directly upon the Cretaceous. Marine on the coast of Provence, west of Marseilles. Brackish farther inland, where one of the most characteristic deposits is a limestone with *Helix Ramondi.*

Miocene.

II. *Faluns of Sausset* and *Sands with Scutella paulensis.*—Conformably succeeds the Aquitanian. On the coast, light coloured, sandy and gritty beds ("Faluns of Sausset") with numerous gasteropods, *Pecten vindascinus*, etc. Farther towards the north the typical facies consists of sands with *Scutella paulensis;*

Miocene.

Pecten occurs and the deposits are clearly marine, but fossils are not common. Does not extend farther north than the valley of the Drôme.

III. *Marly and calcareous molasse* with *Pecten praescabriusculus.*—Extends from the coast up the valley nearly as far as Montelimart; and there leaves the river altogether, running up as a narrow band towards the north-east into Savoy.

IV. *Sandy molasse.*—Marine sands and sandstones. Covers the whole region occupied by III. and extends also farther to the west. Its western boundary closely follows the borders of the Central Plateau. Beds like the Austrian "Schlier" occur near the base.

V. *Upper molasse* with *Cardita Jouanneti.*—In the centre of the Rhone Valley IV. is succeeded by a return of marly and calcareous deposits; but these are less extensive and are absent to the north of Lyon and in Switzerland.

VI. *Upper brackish, fresh-water and fluviatile Miocene.*—The last mentioned series (V.) forms the close of the marine Miocene and it is succeeded by brackish and fresh-water beds. These are absent in the greater part of Provence and do not extend farther south than Aix.

VII. *Beds* with *Congeria sub-Basteroti.*—Occur only locally. None of the species agree with those found in the Congeria beds of Austria and the author places them at a higher horizon.

Thus it appears that at the close of Oligocene times the sea washed the shores of Provence: but it did not extend farther inland than at present. The valley of the Rhone was occupied by a series of lagoons lying along the foot of the sub-Alpine chains. During the progress of the Miocene period the sea spread up the valley, and the gulf so formed continued to increase in size up to the time of the deposition of the beds of Stage IV. After this it again retreated, and during the later stages we have a return of the lagunary conditions which prevailed during the earlier part of the period.

In Austria the lower part of the Miocene was long ago divided by Suess into a first and a second Mediterranean stage. But later observers (Tietze, Bittner) have considered that these are merely lateral facies of the same stage. Depéret, however, agrees with Suess and groups the beds as follows:—

First Mediterranean Stage.—Except the uppermost bed, exists only outside the Alpine zone north of Vienna, and not within the Vienna basin proper. Rests directly upon the crystalline rocks of Bohemia and Moravia.

1. Brackish beds of *Molt.*—Marls and sands. Probably Aquitanian, or the extreme base of the Miocene.
2. Sands of *Loibersdorf.*—Mostly marine, but still contain beds with *Potamides*, etc.
3. Sands of *Gauderndorf.*—Rich marine fauna; in general identical with that of Stage II. in the Rhone Valley.
4. Marly calcareous horizon of *Eggenburg.*—Overlaps the last. Contains *Pecten praescabriusculus* and represents Stage III. of the Rhone.
5. *Schlier.*—Blue clays with sandy beds. Molluscan fauna poor. Probably the equivalent of the lower part of IV.

Second Mediterranean Stage.—Rests upon the Schlier in the extra-Alpine part of the Danube basin and with the Schlier penetrates into the intra-Alpine or Vienna basin properly so called.

1. Horizon of *Grund.* Sands and clays with marine fauna, including some species of the first stage. A few brackish forms. Probably corresponds with the upper part of IV. in the Rhone Valley.
2. *Upper beds.*—Deposits of very varied character, the relations of which are not clear. Depéret believes that he can distinguish two assises, *viz.* :—
   (*a*) *Leithakalk.*—Marly limestones with sandy bands. Largely made up of nullipores, bryozoa, foraminifera, etc. Littoral.
   (*b*) Clays of *Baden.*

Sarmatian.—Brackish and uniform fauna characterised by abundance of *Cerithium. Trochus podolicus*, *Donax lucida*, etc., are characteristic forms.

Pontian.

1. *Beds* with *Congeria sub-globosa.*—*Melanopsis*, *Congeria.* Brackish.
2. *Belvedere* gravels.—Fluviatile. Sands and pebbles which form a kind of carpet over the whole basin.

Hence in Austria also the series commences with brackish deposits; and the beds become more and more marine in character till we reach the second Mediterranean stage; while the Sarmatian and Pontian denote the return of brackish and, finally, of fluviatile conditions.

In Switzerland we have :—

A. *Aquitanian.*—Brackish; with *Potamides*, *Helix Ramondi*, etc.
B. *Grey molasse of Lausanne.*—Fresh-water sands with a rich flora. The mammalian remains show a certain approach to the Oligocene type.
C. *Marine molasse.*—The Lausanne beds pass up insensibly into sandstones which contain *Pecten praescabriusculus*, etc., in the

lower beds, etc., *Cardita Jouanneti* in the upper. Clearly marine: represent Stages III.-V. of the Rhone Valley.

D. *Upper fresh-water molasse.*—(Oeningien). Sandy at the base, marly and calcareous above. *Helix moguntina*, and *H. sylvana* are especially characteristic. In the Rhone Valley these forms occur, not in the upper fresh-water beds (VI.) but intercalated among the marine beds below, with which therefore the Swiss D. must be correlated.

Thus in Switzerland there was a similar incursion of the sea during the middle of the Miocene period; but it began later and ended earlier than in the basin of the Rhone.

The Swiss Miocene is continued to the east into Wurtemberg and Bavaria, where two bands can be recognised. The northern extends from Constance to the east and north towards Ulm, and in it the beds are almost undisturbed. The southern passes to the south of Lake Constance through the Vorarlberg and the southern part of Bavaria into Austria; and in this band the beds have shared in the foldings of the Alps. Here also the same general sequence can be made out, and the marine beds occur in the middle of the Miocene.

M. Depéret also briefly notices the Miocene of Italy[1] and of Northern and Western France, and finally gives a summary of his results in the form of a table of correlation.

Returning to Austria, in the Carpathians and the province of Lower Austria the Cretaceous (or the upper Cretaceous[2]) and the lower part of the Tertiaries are represented by a thick mass of sandstone, which is generally unfossiliferous and is commonly known as the Vienna or Carpathian sandstone. This sandstone extends towards the north-west into Moravia, where it forms the Steinitzerwald and Marsgebirge, which run from west-south-west to east-north-east and form a kind of sub-Carpathian chain; and here it has been examined by Paul (26).

[1] Reference may here be made to a paper by Sacco (30), which gives a short account of the Tertiaries of the Turin Hills.

[2] In an appendix to his paper (26) Paul maintains, in opposition to Uhlig, that the lower Cretaceous also is represented.

The lowest Tertiary division of the Carpathian sandstone in this region is the orbitoides breccia, which is exposed near Auspitz in the centre of an anticlinal. It has sometimes been called "nummulite sandstone," but it contains *Orbitoides* and not *Nummulites*. Excepting for this the main mass of the Steinitzerwald is made up of the next succeeding bed, a light-coloured platy sandstone called the Steinitz sandstone, which near Auspitz passes into marls (Auspitz marls). The so-called "upper hieroglyph beds" and the well-known Menilit-schiefer (Amphisyle-schiefer of Suess, with fish-remains) are really local facies of the same deposit; and lenticular masses of clay containing foraminifera also occur locally. No exact correlation can be attempted, and all that can be determined is that these beds belong to the older Tertiary.

The newer Tertiaries are represented by the Magura sandstone ("March-sandstein" of Glocker), which forms the main mass of the Marsgebirge. It is generally a coarse and unequally-grained quartzose sandstone, often breccia-like at its base; and fossils are rare within it.

Thus the series as developed here consists of:—

Older Tertiary.	Orbitoides beds. Steinitz sandstone (including the Amphisyle beds, Auspitz marl, etc.).
Newer Tertiary.	Magura sandstone.

These beds are thrown into a series of anticlinal and synclinal folds, the axes of which run from south-west to north-east, parallel to the direction of the Carpathians in this area.

The lower ground to the north-west, between these mountains and the older rocks north-east of Brünn, has been examined by von Tausch (38), who devotes the greater part of his paper to the description of the Miocene deposits. These all belong to the second Mediterranean stage and consist of sands, marls, clays, etc., with nullipore limestones and sandstones in the upper part. Excepting locally the beds are horizontal: to the west they probably rest directly upon the Culm, to the east upon the Flysch of the Carpathians.

Somewhat farther towards the north, Toula (39) notes the occurrence of Miocene at Kralitz, near Prossnitz. There are two series, the lower of which consists of yellow or greenish marls (Schlier) and contains masses of *Pecten denudatus;* while the upper is composed of loose marls with numerous fossils. In these upper beds foraminifera, bryozoa and echinodermata predominate; and of the foraminifera there are no fewer than 110 species.

South of the Steinitzerwald, near Saitz, Redlich (27) has obtained a number of Miocene fossils, the most abundant of which are *Ancillaria glandiformis* (which occurs in thousands), *Cerithium crenatum*, etc.

In Styria the border of the great Austrian basin has been examined by Hilber (16), where, to the west of Gratz, the Tertiary deposits rest upon the schists of the Eastern Alps. The Miocene commences with a lacustrine facies, consisting of clays with beds of lignite, and some limestones. These belong to the first Mediterranean stage and are succeeded by the Sarmatian, which includes limestones, sands and clays with *Cerithium rubiginosum*, *Trochus podolicus* and other typical Sarmatian fossils. The eastern part of the area is occupied by the Pontian, which is chiefly remarkable for the number of its plant remains; while the succeeding Thracian or Belvedere beds (upper part of Depéret's Pontian) are of the normal character and contain *Aceratherium incisivum*, *Mastodon longirostris*, etc.

In the South-west of Russia there is a very extensive area of Tertiary deposits, and these have been examined by Ivanova (17) along the left bank of the Dniester in Podolia. His remarks are directed towards the Sarmatian, the fauna of which he describes; and he has been led to divide this stage into a number of horizons according to the species of *Cerithium.*[1]

In the western parts of Turkey in the province of Albania, there is an extensive basin of Tertiary deposits, which extends from Kanina to the south of Avlona and

[1] These horizons are not very distinctly defined. The Cerithia appear to have changed very gradually.

inland as far as Berat. It occupies, in fact, a roughly triangular area where the higher mountain chains recede a little from the coast. The whole of this basin has been referred by Coquand to the Pliocene. The lower beds consist of bluish clay, and the upper mainly of sands and conglomerates with *Janira jacobaea*, etc. Simonelli (34) has recently examined the fossils which come from a sand near Selenitza (evidently belonging to the upper series), and his list confirms Coquand's views. Excepting *Potamides pictum*, which is a Sarmatian form, all the fossils are common Mediterranean Pliocene forms; and they indicate deposition in brackish water.

QUATERNARY.

Concerning the post-pliocene deposits there is but little to say. Wahnschaffe (40) describes the occurrence, near Berlin, of a Paludina bed [1] below the lower boulder clay, made up largely of *Paludina diluviana*. This species now lives in the lower part of the Danube in Dobrudscha and it has been found as an erratic in the lower boulder clay of Germany. Its presence there was difficult to explain until it was shown that it had lived in that country before the glacial period.

In the province of Basilicata, in the South of Italy, Lorenzo (21) notes the existence of morainis on the mountains of the Sireno group, showing that the cold of the glacial period was felt even as far south as this.

Remains of the pre-historic period in France have been noted by Tardy (37) at Maz d'Azil in the department of Ariège and by Givod and Massénat (15) in the valley of the Vézère in Dordogne.

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THE LOCALISATION OF ENZYMES IN PLANTS, WITH SOME CONSIDERATIONS AS TO THE DIFFERENTIATION OF GLANDULAR STRUCTURES.

IN the processes of nutrition which are observable in the vegetable organism, two essentially different modes of procedure have made themselves prominent. In the great majority of plants we find constructive methods most easily visible. The green plant, taking in extremely simple substances by its leaves and its roots, builds them up into more complex ones, till such substances are prepared as are suitable to nourish the living protoplasm. These processes, which we may speak of as constructive, are, however, intermittent. The decomposition of CO_2, and the subsequent elaboration of food material, takes place under the combined influences of chlorophyll and sunlight. As soon, therefore, as these cease to co-operate, the processes of construction cease. We have such intermittence caused by the alternation of day and night, and by the changes due to the sequence of the seasons. As, however, life continues in the organism, in spite of this intermittence, and as the vital processes necessitate the continuous nutrition of the living substance, it is evident that there must be a further accessory series of operations to supply the necessary pabulum when construction is in abeyance. While it is active, much more elaborated material is prepared than is needed for the time being, and the surplus is deposited in the tissues temporarily, forming a fund on which the organism may draw in the intervals of such construction. We find, consequently, in plants, reservoirs of nutritive material of various kinds, placed in many different regions; some, in small amount, intended to carry on nutrition during small periods of time; others, containing large quantities, destined to serve the plant during longer intervals. The utilisation of these materials, wherever stored, and for whatever period, is a different process from the first one, involving, instead of constructive processes, those of

decomposition, or splitting up of the very complex substances into more simple ones, capable of travelling from the centre of deposition and of immediate assimilation. We have, indeed, the process typically shown in the animal body, a true digestive act. Moreover, in plants as well as animals this digestion is carried out by the agency of enzymes or ferments. These bodies, indeed, with the doubtful exceptions afforded by the so-called carnivorous plants, seem to be formed entirely with a view to the utilisation of the reserve materials, deposited as we have seen in the plant tissues, when constructive metabolism is in excess of immediate requirements.

In seeking to determine the distribution of ferments or enzymes in the plant tissues it is advisable first to ascertain where the reserve materials are deposited, and in what forms. In many cases this is extremely easy. Seeds, tubers, bulbs, fleshy stems and roots may be seen to contain them in large quantity. The cells are frequently found to be crowded with starch grains, drops of oil, or proteid materials, in the form of aleurone grains; the cell-sap may be found to contain large quantities of soluble carbohydrates, such as sugar or inuline. In certain cells in the wood of trees the winter supply may be demonstrated to exist in various forms, of which starch is the most widely distributed. In other cases the detection is more difficult; complex glucosides are found by proper treatment to be present in more restricted localities. Compounds of phosphorus may be detected stored up in definite layers of cells in particular seeds (1). All these forms are intended to nourish the plant during seasons of inactivity lasting for some time. From certain recent researches (2), it appears that the starch grains that are so constantly seen in the chlorophyll corpuscles of the leaves during vigorous assimilation, are equally reserve materials, intended primarily to nourish the plant during the period of darkness, or to be transported later to more permanent reservoirs. In the very simplest plant, whose body is composed of only a single cell, these materials may be found to be stored up in the protoplasmic contents, and to disappear at particular times.

Somewhere in the neighbourhood of these reserve materials therefore, sometimes in the very cells containing the reserves, the enzyme may be expected to occur. Where not in the cells themselves, it might well be supposed that the differentiation of internal tissues would lead to the localisation in special cells of the agent needed for digestion.

In attempting to identify the localities, two methods of procedure are open to the inquirer. The microscope will show their presence, if they are sufficiently distinctive to present any peculiar structure or reactions. Or their presence may be ascertained by putting pieces of tissue which are suspected to contain them, in such conditions as will lead to their action, and the formation of recognisable products.

Both these methods present considerable difficulty, but both have been attempted, and with more or less success. In the case of animal cells, which are most easy to examine by the methods familiar to the histologist, the protoplasm of secreting gland cells has been found to show a peculiar and characteristic granularity, and the amount of the granules has been found to vary *pari passu* with the amount of ferment that can be extracted from them. When applied to vegetable histology, however, the granularity fails to be a guide on which reliance can be absolutely placed. The vegetable cell usually contains relatively little protoplasm, and the materials found to be in solution in the cell-sap interfere very much with the processes of staining.

It has, however, been shown, though in somewhat general fashion, that where a vegetable tissue is the seat of an active zymotic power, the cells are found to possess a markedly granular protoplasm. Gardiner (3) pointed this out in the cells of Dionæa, the Venus's fly-trap, where the glands are comparatively isolated in position. Brown and Morris (4) showed it to be the case in the epithelium of a particular part of the seed of the grasses, from which they extracted two different enzymes. Marshall Ward (5) found coarse granularity in certain fungi, when ferment could be proved to exist in the same hyphæ as the granules; other observers have found the same appearances in pollen tubes

(6). Guignard has noted similar granularity in cells proved by other means to contain emulsin, in the almond and the laurel (7). In the cells which cover the embryo in various palms, and which eat into the horny endosperm in which this structure is impacted, the same granularity may by appropriate treatment be made visible.

Mere granularity, however, is not a sufficient test for the existence of the enzymes. Various staining reactions have also been used, though not with uniform results. Wiesner (8) nearly ten years ago advocated the use of an alcoholic solution of orcin, supplemented by boiling the tissue with strong hydrochloric acid. He obtained thus a definite coloration of the contents of the cells in which various ferments occurred, the colour varying with the nature of the ferment. By this method he thought he would be able to determine, not only the presence of an enzyme, but to identify it. The method was later shown to be inconclusive, as many other bodies give similar colour reactions. It cannot therefore be relied on, unless the cells examined are known to contain none of these, a very difficult matter, as most of the known carbohydrates are among the bodies which give the reaction. Hydrochloric acid by itself gives distinctive colours with the different enzymes ; diastase turns red, and later on brown, emulsin becomes violet, papain orange-red, trypsin greenish-yellow. Again, however, experiments with other bodies show by their behaviour that the results are not to be relied upon as distinctive. The cells generally contain, in addition to the enzymes, considerable quantities of proteids, and these are affected in various ways, giving rise to colour reactions that resemble those first described. It thus becomes difficult or impossible to say with confidence what body is the one to which any particular colour reaction is due. Though we cannot give any general test of this kind for the presence of an enzyme, yet particular ones may be detected by careful experiments. The most successful investigations in this direction are those which have recently been carried out by Guignard (9) on the glucoside-splitting ferments yielded by the Cruciferæ and the Rosaceæ. He detected them first in the leaf of the cherry laurel.

The cells of a particular region of the tissue are marked by the finely granular character of their protoplasm, and their freedom from starch or chlorophyll, though a certain amount of tannin is present in them, and they are richer in proteids than the other cells of the leaf. Using Millon's reagent, which is a mixture of the two nitrates of mercury, all the cells of the leaf blackened, but the colour was deepest in those in question. On warming, the black tint gradually disappeared, and was replaced by an orange-red in the cells containing the enzyme, and by a faint pink in the ordinary parenchyma. In an allied species, *Cerasus lusitanica*, which contains tannin, but not enzyme, in the corresponding cells, this orange-red coloration was not found to be developed by the same treatment. The reaction was not therefore due to the tannin, but to some peculiar proteid constituent with which the enzyme is associated, or to the enzyme itself, probably the former, as it resembles fairly closely the ordinary proteid behaviour with Millon's reagent. By the presence of this proteid body, and its response to the test, the enzyme-containing cells could be recognised. Confirmatory tests for proteids bore out this opinion. $CuSO_4$ and KHO coloured these cells a violet-pink, while cells containing only tannin did not react to it more than ordinary parenchyma cells with their lining of protoplasm, which stained a pale pink without admixture of violet. Similar experiments led to the recognition of certain special cells in various tissues of the horse-radish and other cruciferous plants (10) which stood out in marked contrast from their neighbours. They became orange-red, while the ordinary parenchyma in which they were embedded only took on a pale pink tinge. With Fehling's fluid these could also be distinguished. The proteid contents could, with a little care, be distinguished from the protoplasm of the cells in which they were found. Treatment with alcohol of 50 per cent. strength caused the whole contents to shrink a little from the cell wall, and the proteid granules shrank from the protoplasm itself, so that the contents and the protoplasm could be seen separately. The latter was coloured orange-red by subsequent treatment by Millon's reagent, while

the protoplasm was only tinted pink. Various aniline dyes also stained the granules. The determination and identification of the proteid did not, however, establish the existence of the enzyme. Subsequent experiments of a different character were necessary to show its presence. These, however, did demonstrate the fact of its occurrence with the proteid, so that the latter formed a test for the ferment, and enabled its whereabouts to be ascertained.

The second line of investigation indicated above, *viz.*, the obtaining of evidence of the action of the ferment, by putting the suspected tissues into the necessary position for such activity, also presents special difficulties. Such a line was followed by Guignard in the experiments last referred to. The difficulties are mainly mechanical, and consist of the delicate manipulation necessary to isolate the cells which are suspected to contain the enzyme. In the laurel leaf these cells, as we shall see later, occur in a sort of band or sheath round the fibrovascular bundles, and it is possible to cut sections of the leaf consisting of but little besides this sheath. The property possessed by this ferment, *emulsin*, is the decomposition of the glucoside *amygdalin* into sugar, benzoyl-aldehyde and hydrocyanic acid, the latter two of which possess characteristic odours. Thin sections taken, including the cells of the sheath, were kept for a short time in a 1 per cent. solution of amygdalin at 50° C., and speedily the peculiar smell gave evidence of the action of the enzyme. Instead of sections being made through the tissue in which the sheath lay, the sheath itself was afterwards dissected carefully out and treated similarly, when very rapidly the odour made itself evident.

The same method of treatment was adopted in testing for the enzyme in the Cruciferæ. This ferment is *myrosin*, and the glucoside on which it acts yields on decomposition sulphocyanate of allyl, the aromatic principle found in oil of mustard. The special cells which give the reactions already mentioned can be separated in a definite layer in the wall-flower, and the piece of tissue so dissected out causes the evolution of this odour when digested for a short time with a solution of the glucoside.

This method, in some cases, yields very striking results, although the preliminary difficulties of dissection are very great. One of the most successful instances of its application is given by Marshall Ward (10) in his experiments upon *rhamnase*, an enzyme causing decomposition of a glucoside present in the fruit of *Rhamnus infectorius*, the so-called "Persian berry". This fruit yields a valuable dye of a bright yellow colour, which is one of the products of the splitting up of the glucoside. On preparing a solution of the glucoside from the pericarp of the fruit, and putting a small piece of the suspected tissue upon its surface, so that it floated, the action was visible in the course of a few minutes. In ten minutes the floating piece of tissue was covered with a golden-yellow precipitate of the colouring matter; in twenty minutes clouds of the same precipitate were sinking through the solution, and in less than an hour the bottom of the test tube which contained it was covered by a layer nearly two millimetres deep.

Nor is it only with glucoside ferments that this method has succeeded. Brown and Morris (4) detected diastase in particular cells by isolating plates of them, and warming them on the surface of gelatinised starch paste. Where the particular cells rested, the starch became liquefied and sugar was formed. Strasburger showed the presence of the same body in pollen tubes, by allowing them to grow in solutions containing starch, when again hydrolysis took place.

In cases where the enzyme exists side by side in one cell with the body in which it works, its presence may be noted by warming the tissue containing them to about 40° C., for some time. Microscopic examination will then be sufficient to show where the action is taking place; if the body be solid, as in the case of starch or aleurone, the disappearance of the grains will at once be evident; if it be in solution, as in that of sugar or inuline, micro-chemical tests can easily be applied.

By these various means the locality of the ferment-containing tissue can in most cases be satisfactorily established, and a study of its distribution renders evident a variable degree of differentiation, extending from the simple

specialised vegetable cell to a complex and well-marked tissue, corresponding closely in structure and function to the secreting gland of the animal body.

In the simplest cases of secretion we find the ferment formed in the same cell as its appropriate reserve material. Such may be seen in the pollen grains of many plants (11), and in various unicellular fungi. Many pollen grains have been shown to contain either diastase or invertase, or both, and in them simultaneously may be found starch, or cane sugar. In other cases the unicellular organism does not contain such reserve materials, but usually lives in, or upon, a medium which does. The ferment formed by the plant is then excreted into the medium, performs its work there, and the cell absorbs the products of its activity. Such excretion has been shown in the life history of many bacteria, some indeed showing the power of forming different enzymes according to the medium on which they find themselves (12) growing. In fungi of a little higher type the course of procedure is the same, but the secretion of the enzyme is confined to particular parts of the much branched cell, or to particular, generally terminal, cells of the mycelium. Turning to the higher plants, we find in the pollen tube a similar formation of ferment, which is extruded at definite places, sometimes by well-defined pores into the tissue through which the pollen tube is growing, and which contains the appropriate reserve materials.

In these comparatively lowly growths, the absence of differentiation renders it extremely difficult to say much about the localisation of the enzyme. Probably it is coterminous with the cell protoplasm, though more of it may be at one part of the cell than another. In higher plants, with great complexities in the arrangement of tissues, more approach to specialisation can be seen.

We find conspicuous cases of very wide distribution even among these higher forms. Thus, diastase has been determined with some precision to exist almost everywhere in the young and green parts of nearly all plants (13), besides being specially marked in parts hidden from the light, such as seeds and tubers. This enzyme varies very much in

quantity, not only in various parts of a plant, but in the same part at different times. In the resting potato tuber, for instance, little can be detected; but as germination begins, the enzyme makes its appearance close to the growing part, and later, appears regularly farther in the interior (14), till it can be found throughout its substance. Diurnal variation has been noted in the case of leaves, more being found in the early morning than at sunset, the quantity gradually increasing as darkness advances (2).

Diastase is, however, the only enzyme which has such a wide distribution. Most other reserve matters than starch are deposited in very definite parts of the plant, and their appropriate enzymes are generally restricted to similar confined areas. The enzyme that most resembles diastase in being found in a variety of tissue is vegetable rennet, though the latter does not occur, as does the former, in nearly all plants. We find rennet in seeds, both during and before germination; in fruits and flowers, even in the bast of certain stems. It is difficult to explain its widespread presence, because we do not yet know exactly what decomposition it sets up. Other enzymes, whose working is more fully known, are much more limited in their distribution. We begin to find a glandular tissue hinted at, though the same cells often function as storehouses. Thus the proteolytic ferment may be found in the cells of the seeds of various members of the natural order Leguminosæ, particularly in those of the cotyledons (15), where it extends very uniformly throughout the cells. There is not in such a structure a differentiation which can be detected in more complex cases where a gradual process of digestion can be seen to spread from certain definite starting-points. An irregular or patchy digestion can be seen in the disappearance of the oil from the endosperm cells of Ricinus communis (16).

This rudimentary differentiation does, however, lead up to a higher one, where certain cells secrete enzyme only, and in tracing this increase of complexity, we can note it taking place very gradually. We can notice, first, as a stage just beyond the one last described, cases where special

ferment-secreting cells occur in the midst of those containing reserve materials, but not possessing any themselves. This has been noted with wonderful care, patience and precision, by Guignard (7), (9) and (17), in his researches on the Cruciferæ and other natural orders, which have already been alluded to. The first case in which successful investigations were made was the root of the horse-radish. Using the micro-chemical tests that we described above, special cells were detected among the parenchyma of different regions, which indicated the enzyme they contained by the character of their protoplasm and their contents. They formed no regular bands of tissue, nor were they usually grouped together; in transverse diameter they were about the same as those in which they were embedded, though they were a little larger as seen in longitudinal sections. In various parts of the root they varied in size, shape, and degree of aggregation.

A very prolonged and painstaking series of researches has given us valuable results in localising these secreting cells. Begun originally on the root of the horse-radish, they have been extended throughout the natural orders, Cruciferæ, Capparidaceæ, Resedaceæ, Tropæolaceæ, and Limnanthaceæ, and the various regions of typical members of all these groups have been carefully scrutinised. One fact comes out in them all, and in all parts of them, that the ferment and the glucoside on which the ferment works are always enclosed in different cells. The distribution may be briefly summarised as follows:—

Roots.—Chiefly in the cortex, but sparsely in the wood. In fleshy roots the tissue representing the wood is mainly parenchymatous, and contains them. In woody roots they are found in the secondary bast as well as in the cortex, and to a less extent in the medullary rays. In the Capparidaceæ the wood contains none, but some are found in the pith. In the Resedaceæ they do not occur farther inwards than the bast.

Stems.—There is a good deal of variety observable in different species. Speaking broadly, the pericycle

and the tissue derived from it are their chief seat, and next in importance is the secondary bast. When they occur in the wood, it is generally in the medullary rays. The pith also shows their presence. Guignard makes nine types of distribution in the stems of Cruciferæ :—

(1) The pericycle alone, *e.g.*, Lepidium sativum, etc.
(2) The pericycle, and the primary and secondary bast, *e.g.*, Erysimum cheiranthoides.
(3) The cortex chiefly, but also the pericycle, *e.g.*, Moricandia hesperidiflora.
(4) The cortex and the bast, under a thin and sclerenchymatous pericycle, *e.g.*, Iberis amara, etc.
(5) The cortex, pericycle, and pith, *e.g.*, Nasturtium officinale, etc.
(6) The cortex, pericycle, and secondary bast, *e.g.*, Bunias orientalis, etc.
(7) The cortex, pericycle, secondary bast, and pith, *e.g.*, Raphanus sativus, etc.
(8) The cortex and pericycle chiefly; also the primary and secondary bast and pith, *e.g.*, Brassica nigra, etc.
(9) The cortex, pericycle, primary and secondary bast, woody parenchyma and pith, *e.g.*, Cochlearia armoracea, etc.

The distribution is somewhat similar in the other families referred to. In the Limnantheæ they lie mainly in the lacunar cortex, but a few are to be found in the bast; in the Tropæolaceæ they generally form groups or nodules of cells in the hypodermal layer of the cortex, a few being in the bast.

Leaves.—Species that contain many of the secreting cells in the axis of the plant generally contain them in the leaves also, and the relative proportion is often greatest there. They occur throughout the mesophyll, but are usually most numerous towards the lower surface. In some leaves they are localised in the mesophyll and the pericycle; in others, chiefly

in the pericycle and the bast of the veins. In a few cases they occur in the endodermis of the bundles. In the Capparidaceæ they are found two or three together, the groups extending throughout the parenchyma.

Flowers.—In Capparidaceæ the flower contains large numbers of the secreting cells in both sepals and petals. They are most numerous in the pulp of the fruit. In this order they are arranged in the flower and fruit into little groups, each of which is developed from a single cell which becomes prominent while the tissue is still very young. In Tropæolum they are very prominent in the tissue of the spur. In the flowers of Cruciferæ they are chiefly met with in the carpels, particularly in the neighbourhood of the fibrovascular bundles.

Seeds.—In Cruciferæ the integuments of the seed alone, or both embryo and integument may contain them. Guignard distinguishes five types of localisation :—

(1) The cotyledonary parenchyma and the cortex of the axis of the embryo.

(2) The tissue abutting on the back of the cotyledonary bundles, or the pericycle if that layer is differentiated ; also in the cortex of the axis.

(3) Both regions described under (1) and (2).

(4) Sometimes they are absent from the radicle and the cotyledons.

(5) The integument only.

In the Limnantheæ these cells can be detected in the parenchyma of the cotyledons when the seed is very young, and before any reserve materials are stored in any of the cells. Later, on germination, they can be recognised in the lower epidermis when the cotyledons become green. The other orders show a similar distribution.

Guignard also ascertained the distribution of emulsin, the ferment of the almond and laurel. In the leaves and young branches of the latter plant the endodermis of the

bundles are its chief seat, certain cells of the pericycle also probably containing it. In the almond emulsin is localised chiefly in the pericycle of the cotyledonary bundles.

A curious distribution of a similar ferment, rhamnase, was ascertained by Marshall Ward (10). It occurs in the seeds of Rhamnus infectorius, and is confined to the raphe of the seed. In this, as in the preceding cases, the glucoside and the ferment are not found in the same cells.

In reviewing these cases, in which apparently rudimentary glandular tissue is being differentiated, we see a tendency to aggregate the secreting cells together. Indeed, in some of the cases quoted, the tissue is produced by the continued division of cells originating from a single primitive one, as in the fruit of Capparidaceæ and the cortex of the stem of Tropæolum. A more fully differentiated structure is seen in the scutellum of certain of the grasses, and the absorbing haustorium of the cotyledon of the palms, which remains embedded in the endosperm while germination is proceeding. The scutellum of the grasses is covered by a delicate epithelium (4), one cell thick, which is specially adapted for secretion, and which puts out two enzymes into the endosperm to work upon the cell walls and the starch grains respectively. The histological changes which these cells show as germination progresses, and their activity varies, strikingly recall the behaviour of the animal gland cell. The epithelium of the Palm haustorium has not been proved to excrete enzymes, but its appearance during germination is very similar to that of the scutellum, and its growth is attended by a corrosion or eating away of the tissue of the endosperm which is very much like that observed more easily in the grasses.

Finally we have the well-differentiated secreting glands (3) of Dionæa (18) and Drosera, where we find tissue specially set apart for the production of peptic enzymes, intended to work upon substances exterior altogether to the plant's body.

These recall the animal enzymes not only in the mode of their secretion and the position of the materials they digest, but also in the fact that the enzyme is accompanied by an

acid in the secretion, and that both acid and enzyme appear to be necessary for the process of digestion. Nepenthes has hitherto been held to show glands belonging to this group, but recent experiments seem to bring their behaviour into the region of controversy.

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J. REYNOLDS GREEN.

RECENT ADDITIONS TO OUR KNOWLEDGE OF THE ANCIENT SEDIMENTS.

SINCE the publication of the map and writings of William Smith there have been two particularly important events in the progress of our study of the older sediments, the first being the elucidation of the structure of North Wales and the Welsh borderland by Murchison and Sedgwick, and the second the publication of Lapworth's memoir on the Moffat series (1).[1]

Lapworth's paper marked the commencement of a new era in the study of our ancient sediments, and, as it exercised a profound influence upon the work which has been achieved in recent times, it will be convenient to take it as our starting-point, when attempting a sketch of the progress of our knowledge of the older rocks during the last few years. This progress has been made in two ways: firstly, by furnishing us with more accurate information of the methods of stratigraphical geology, and, secondly, by enabling us to apply our knowledge of the strata to the elucidation of geological problems with greater success. In giving a brief outline of recent advance, I propose to dwell, in the first instance, upon the methods of stratigraphical geology, and afterwards upon the application of knowledge thus acquired.

METHODS OF STRATIGRAPHICAL GEOLOGY.

The recent work which has borne most fruit is due to a more detailed application of Smith's principle of strata being identifiable by their organised fossils—a principle which, until the publication of Lapworth's work on the Moffat series, had been viewed somewhat askance, owing in great part to the publication of the theoretical views of Whewell, Huxley and Spencer, which were readily adopted

[1] The numbers refer to the list of papers appended to this article.

by those who made no detailed examination of the fossiliferous strata of the earth's crust. The result of the adoption of these views was that the study of stratigraphical geology remained for some time in abeyance, a circumstance by no means to be regretted, if, as is likely, it was the direct cause of the diversion of geologists' attention to the constitution of rocks, which has resulted in the magnificent advances in petrographical science during the last two decades.

We may at once proceed to treat of various ancient rock-systems, beginning with the lowest, in order to see what advance has been made in our knowledge of them during the last few years.

1. PRECAMBRIAN ROCKS.

Although the existence of Precambrian rocks in Britain was long ago satisfactorily demonstrated, it was not until the establishment of the *Olenellus* zone as marking a definite horizon that our knowledge of these rocks in Britain was placed on a satisfactory standing, and even now acquaintance with these rocks can only be said to have commenced, and a classification of the Precambrian rocks of Britain, and indeed of other parts of the world, is at present premature, for we cannot say how much of the material which enters into their composition was originally igneous, and therefore newer than rock which now lies above it; chronological classification of these rocks, accordingly, though it has been frequently attempted, is of little value. One thing has now been clearly established, namely, the existence of sedimentary rocks in Britain and elsewhere underlying the *Olenellus* beds of Cambrian age. Thus, in 1890, Sir A. Geikie (2) was able to announce the Precambrian age of the Torridon sandstone of North-west Scotland, and in the next year he told the Fellows of the Geological Society, in his presidential address (3), that the officers of the Geological Survey of Scotland had obtained proofs of the existence of much sedimentary material in the South-

west Highlands amongst those rocks which he claimed as largely Precambrian, and to which he gave the name Dalradian. Similar sediments have been proved of Precambrian age in many extra-British areas, our knowledge of them in America being fairly extensive, owing to the recent work of Van Hise, Lawson, Irving, Walcott and others. In America, as in Britain, chronological classification cannot be carried very far at present, and the majority of American writers are satisfied with separating undoubtedly bedded Precambrian rocks, under the name of Algonkian, from those of igneous and doubtful origin, for which they retain the name Archæan. An admirable summary of the present knowledge of these American rocks has been recently given by Van Hise (4). Further consideration of the Precambrian rocks may be deferred until we consider, in the latter part of this article, the application of our knowledge of stratigraphical principles.

2. THE CAMBRIAN SYSTEM.

The establishment of the position of the *Olenellus* zone by Nathorst (5), Brögger (6)and others has given us a satisfactory base to the strata appertaining to the Cambrian system, and the sequence of Cambrian faunas is now firmly established. It is generally admitted that the *Olenellus*, *Paradoxides* and *Olenus* faunas succeed each other in definite sequence, and the only want of accordance amongst geologists concerning the rocks of this system is on a matter of classification, the foreign geologists placing the beds with the Tremadoc fauna in the succeeding system, whilst British geologists relegate them to the Cambrian. As the fauna constitutes a true passage between those of the Cambrian and Ordovician systems, the question is one of small importance. Undoubtedly, the greatest advance in our knowledge of the rocks of the Cambrian system of recent years is due to the detection of the *Olenellus* beds in widely remote regions. The splendid monograph upon these beds by Walcott (7) contains a bibliography up to the year 1890.

In that monograph we find a record of the beds as occurring in Britain, Scandinavia, Russia and various parts of North America. Since its publication other areas have been shown to contain a development of the rocks possessing this fauna. Their existence in Britain had been previously shown by Lapworth (8), who has since, however, described the included fossils in greater detail (9). Another British area where *Olenellus* beds occur is in the Scottish Highlands. *Olenellus* has been detected in Ross-shire by Messrs. Peach and Horne (10). Abroad we have now a detailed account of the beds of this age by Bornemann in Sardinia (11), and fossils which appear to appertain to the *Olenellus* zone have been described by Dames in China (12) and Foord in Australia (13), whilst Waagen records the discovery of *Olenellus* by Middlemiss in India (14). Hicks also claims that the Caerfai beds of South Wales should be referred to the *Olenellus* zone (15).

Of the higher Cambrian beds little need be said. Our knowledge of their local development in various areas and of their included faunas has steadily increased. In our own country two important discoveries have been recently made, *viz.*, the detection of *Conocoryphe* in the green slates of Bethesda by Dobbie (16), and the discovery of *Paradoxides* above the *Olenellus* beds of Shropshire by Groom (9). In foreign countries much progress has been made in the detailed working of the Cambrian rocks, with the result that they can now be largely grouped into definite zones characterised by special forms of life. Amongst the valuable work which has been done may be mentioned that of Brögger in Norway (17), Tullberg in Sweden (18), and that which Matthew has published during many years on the fauna of the St. John group in the *Transactions of the Royal Society of Canada.*

3. THE ORDOVICIAN AND SILURIAN SYSTEMS.

As the character of the work which has been recently done amongst the rocks of these two systems is essentially

similar, it may be considered under the same head. It was whilst working at these beds that Lapworth gave the clue to the geology of the Scotch Southern Uplands (1) and established that zonal method of work amongst the older rocks which has led to such excellent results. The use of graptolites as indices of geological horizons has since been fully justified, and the consequence is that our knowledge of the minute subdivisions of the lower Palæozoic strata is now far in advance of that of many rocks which are much higher in the geological column. Lapworth himself extended his observations on the classificatory value of the graptolites to areas remote from his typical Moffat district (19), and similar investigations have since been made in other areas by a number of other observers, amongst whom Tullberg stands prominent (18). As Lapworth remarks, "in the face of these results the host of proofs formerly supposed to be afforded by the abnormalities of the vertical distribution of the graptolithina in favour of the doctrines of migration and colonies vanish into thin air," a statement which was by no means generally accepted at the time it was written (in 1879); but so rapid has been the increase of our knowledge since then that the same writer was enabled to observe in 1889 that the zonal method has "quietly entered upon what may be regarded as the accepted or orthodox stage".

Before leaving the Silurian rocks, reference must be made to our present knowledge of the line of demarcation between the rocks of the Silurian and Devonian systems. Unfortunately, in Devonshire, where the normal marine sediments of Devonian age are developed, there is no passage downwards into the lower Palæozoic strata, whilst in the Welsh borderland, where a passage occurs, the Devonian beds are of the sparsely fossiliferous old red sandstone type. In other areas, the transitional beds are better represented and it is possible to draw a satisfactory line at the top of the Silurian system. The occurrence of *Eurypterus Fischeri* at the summit of the Silurian system in Norway, Gothland, Russia and North America has been commented on by

Schmidt (20), and he places the Lesmahago beds of Southern Scotland in this position.

4. THE DEVONIAN SYSTEM.

The zonal method, applied with so much success to the lower Palæozoic rocks has also been utilised for the elucidation of the smaller subdivisions of the strata of this system, and our knowledge of the classification of the Devonian rocks has recently advanced considerably both at home and abroad. The grouping of the strata in Belgium and Germany has long been established, but until the last few years many opinions prevailed as to the order of succession of the rocks of Devonshire. In 1889, Kayser, the leading authority upon Devonian rocks, described the results of a visit to Devonshire in a paper, "Ueber das Devon in Devonshire und im Boulonnais" (21), and since then Ussher has made a more detailed comparison of some of the Devonian rocks of South Devon with those of the European continent (22), thus practically commencing for Britain a task which will be completed when a thorough re-mapping of North and South Devon and Cornwall has been executed, in accordance with the requirements of stratigraphical palæontology.

The most important work on the Devonian rocks of the European continent is the proof furnished by Kayser that the stages F, G and H of Bohemia, referred by Barrande to the Silurian, really belong to the Devonian system (23), thereby removing a great difficulty in the correlation of the lower Palæozoic rocks of Bohemia with those of other countries, for the stage E can be shown to possess the faunas of all the divisions of the Silurian system from lower Llandovery to upper Ludlow. Kayser was contented to refer the stages F, G and H to the lower Devonian, but Frech gives reasons for considering that lower, middle and upper Devonian beds have their representatives amongst these stages. His correlation of the Bohemian and other Devonian beds with those of the type-deposits will be found

in a paper on the Devonian of the Eastern Alps (24). A valuable account of the Devonian rocks of Spain is given by Barrois (25), who compares them with their equivalents in other areas. The adoption in America of the line of demarcation between Silurian and Devonian, suggested by Schmidt, would settle the vexed question of the age of the lower Helderberg group. Its lowest subdivision, the Waterlime group, with *Eurypterus Fischeri* would thus constitute the top of the Silurian system, whilst its four upper members would be lower Devonian. The minute classification of these American beds will probably be a work of some difficulty, owing to the variations in their character in different regions (26). When it is made, no doubt a much more successful comparison can be instituted between the European and American beds.

The recent work amongst the Devonian rocks shows that in their case, as in that of the lower Palæozoic rocks, not only are palæontological characters of definite subdivisions in many respects identical over wide areas, but also lithological characters. This is especially well seen in the case of the upper and middle Devonian limestones.

5. CARBONIFEROUS AND PERMIAN SYSTEMS.

Although the rocks of these systems are so strongly separated from one another in Britain, it is well known that this separation is only local, and as the carboniferous insensibly pass into those of the Permian system elsewhere, it will be advantageous to treat of the two systems together.

An idea seems to have grown up in the minds of British geologists that the marine deposits of carboniferous age are not separable into zones comparable with the graptolite zones of the lower Palæozoic rocks and the ammonite zones of the mesozoic strata, and it has been inferred that the carboniferous rocks accumulated too rapidly to allow the differentiation of successive faunas. Modern research points to the conclusion that this view is erroneous, and, as

it is a question of considerable importance, some attention may be paid to it. An examination of our text-books shows that a large number of fossils quoted as characteristic mountain limestone forms are derived from the lists given by Phillips in his geology of Yorkshire; these fossils, and many of the so-called carboniferous limestone fossils preserved in our public museums, are derived from the limestones south of the Craven Fault. The district south of that fault is one which has undergone great disturbance, and limestones of very different ages occur in it. One of these is recognised by the geological surveyors as the Pendleside limestone, and from it a great number of fossils have been obtained. It has yet to be proved that this limestone is the equivalent of part of the carboniferous limestone north of the fault and not of one or more of the Yoredale limestones. If it can be shown, and in the writer's opinion it will be shown, that the Pendleside limestone really appertains to the Yoredale, a large number of our supposed typical mountain limestone fossils will be found to come from Yoredale beds, and not from the carboniferous limestone at all! Under such circumstances little astonishment need be felt at the view that our carboniferous strata cannot be divided into zones. As a matter of fact De Koninck and Lohest some time ago proved that three subdivisions existed in the carboniferous limestone itself in the North of England, a lower one containing certain corals and fish, a middle one with *Chonetes papilionacea*, and an upper with *Productus giganteus* (27). Garwood is now working these and the Yoredale beds in still further detail, and when his work is complete, and when Tiddeman has finished the maps and memoirs of the richly fossiliferous country south of the Craven Fault, we may expect to find our carboniferous deposits divisible into zones in a manner comparable with that which holds good among other and (strange as it seems to use the expression) better-known strata.

The probability of subdividing the carboniferous rocks into zones is maintained by Waagen (14), who points out that the carboniferous fossils have not received the careful

attention which has been bestowed on many of those of Palæozoic age. In the memoir, in which these views of Waagen are advanced, much important information is given concerning deposits which are represented in many parts of Europe by anomalous strata, so that the development of typical marine strata of that age is one which is comparatively little heeded by British geologists. According to Waagen the Gannister series, the Pennant series and the Ardwick series, or lower, middle and upper coal measures, are absent in the Salt Range, but a group is developed there which is newer than the coal measures, which he speaks of as Permo-carboniferous. The basal or Talchir stage of this group contains the well-known boulder-beds, the middle or Karharbari stage comprises the middle speckled sandstones, and the upper or Artinsk stage, the lower Productus limestone and the lower division of the middle Productus limestone. The overlying beds are referred to the Permian proper. A perusal of Waagen's memoir will remove difficulties which have attached to some of the American Permo-carboniferous deposits and similar deposits in Spitzbergen, and to the beds containing the celebrated *Glossopteris* flora. Much new information concerning the carboniferous beds is also given by Barrois in his geology of Asturias and Galicia.

As our knowledge of normal marine Permian deposits is still behind that of similar deposits of most other ages, the description of the Permian marine deposits of the Salt Range by Waagen (14) is a matter for congratulation. He correlates the middle division of the lower Productus limestone with the European Rothliegendes and its upper division with the Weissliegendes and Marl slate, the upper Productus limestone having unfossiliferous sandstones at its top, with the Zechstein and Gypsum stage, and passage beds into the Trias. Above the unfossiliferous sandstones are beds with Ceratite limestones at the base marking the advent of the Triassic period.

The above brief outline of recent work amongst the Palæozoic rocks is designed to show the advance made of

recent years in the correlation of deposits of various areas,—a task which is still viewed with some suspicion by certain geologists of the old school ; it is for all that a very necessary task, for, unless it is done, our science will remain in an imperfect stage of advancement, for how are we to write the history of the earth, unless we can refer the various events which occurred to their proper periods ? The methods of work have been commented upon in the foregoing pages, but the work itself has only begun, and a wide field for fresh research in this direction lies open to the stratigraphical student.

RECENT APPLICATIONS OF STRATIGRAPHICAL KNOWLEDGE.

Already a rich harvest has been reaped as a consequence of the zonal method of stratigraphical work. Every branch of geological science, physical and biological, has profited, but it must suffice here to take a few examples of the benefits which have accrued to different branches, and we will consider the effects of recent stratigraphical work upon our knowledge of deposition of sediment, earth-movement, rock-metamorphism, and organic evolution.

The detailed work of Lapworth, in the Moffat area, was followed by an examination of the equivalent rocks in Ayrshire (28), and accordingly a complete comparison could be made between the thin graptolite-bearing deposits of Dumfriesshire and the much thicker deposits crowded with remains of various organisms, which are developed around Girvan. The remarkable changes in the lithological and palæontological characters of the rocks within this short distance throw much light upon the condition of deposit during lower Palæozoic times, and as the result of his Moffat and Girvan work, and an extension of it into adjoining regions, Lapworth gave us that remarkable paper on the Ballantrae rocks (29), in which the local variations in the characters of the deposits are traced from the Pentland Hills to Kirkcudbright. A further important result of the work in the

Southern Uplands was the detection of radiolarian cherts in the ordovician strata (30), the first of a number of such discoveries, which have largely changed our views concerning the conditions of deposit of the strata forming continents, and the relative positions of continents and oceans in past times.

As an illustration of the light thrown by the zonal method upon the nature of past earth-movements, it is hardly necessary to do more than refer to the elucidation of the structure of the North-west Highlands of Scotland (31), and the detection of a completely new type of geotectonic structure (32). Of a very different nature was the movement described by Brögger, which was responsible for the depression of the soft rock in the region which is partially occupied by the Christiania Fjord (33). This work of Brögger's and other discoveries by the same author, to be mentioned presently, are the direct outcome of his detailed zonal work on the thin lower Palæozoic deposits of the Christiania region (17), a piece of work which is a good instance of the light thrown by modern stratigraphical methods upon questions of denudation, deposition, earth-movement, metamorphism, petrographical complexes, and the distribution of life.

The effects of pyrometamorphism on rocks of varying character has also been worked out in regions where a detailed subdivision of the rocks has been previously made. Brögger's work in the Christiania region is an example of such work (17), whilst recently Barrow has drawn some remarkable conclusions from the study of a carefully-mapped region around an intrusion of muscovite-biotite gneiss (34). The Bergen district has furnished Reusch with evidence of the effects of dynamic metamorphism on fossiliferous lower Palæozoic rocks (35), and similar evidence as to the effects of dynamic metamorphism has been collected by Lapworth (36) and others amongst the disturbed rocks of the Scottish Highlands.

In palæontology enough has been recently learned to prove the importance of detailed examination of the fossiliferous strata, and the value of observations in the field to

the working palæontologist. We are probably on the threshold of great discoveries in this department of the science, and it is easy to see the methods of work which will be pursued in the immediate future. A fuller examination of such diverse deposits of similar age as those of Moffat and Girvan will doubtless throw much light on the influence of sediment and climatic conditions on the distribution, migration, variation and extinction of organisms. But it is in the study of phylogenesis that we may expect the most important results: a suggestion as to the methods of research to be pursued in this study is given by Hermann in his inaugural dissertation (37), as far as the graptolites are concerned. We now possess material for the formation of a fairly complete phylogenetic tree for the graptolites, and their biographer will undoubtedly be able to furnish a most remarkable instance of the lines along which evolution has proceeded. The study is unfortunately looked on with suspicion by biologists, on account of our ignorance of the soft parts of these creatures, but as a compensation for this, we do possess a true key of the order of succession of the forms, obtained by actual observation in the field, and not by speculative groping in the study and laboratory. The material for the study of the phylogeny of the trilobites is less complete than that which we possess in the case of the graptolites, but every year is adding to our knowledge of this group, and we may confidently await a mass of information furnished by a combined study in the field and under the microscope of the corals, stromatoporoids and other organisms.

The above are only a few familiar examples of the kind of work which has been done in elucidating geological problems, work which depends for its successful issue upon detailed observations of the order of succession of the strata. Labours of a similar character have been carried out in many parts of the world, and the result is a very marked advance in all branches of geology during the last two decades. And stratigraphical geology, whilst giving valuable assistance to the other departments

of the science, has freed itself from a stigma. In 1883 the reviewer of a text-book remarked that in the eyes of the vast majority of physical geologists, stratigraphical geology "has rather the appearance of an oppressive nightmare of piles of indigestible facts and meaningless strings of names". This is no longer the case. The importance of stratigraphy is becoming more clearly recognised daily, and surely it is a good omen to find the president of the Geological Society reviewing the stratigraphical work which has been published by the society of late years in his presidential address. By the time this article appears the address (38), which was devoted to a consideration of the work of the Fellows of the Geological Society amongst the Palæozoic rocks, will have been published, and readers may there learn that British geologists have been to the fore in studying the earliest sedimentary rocks of which we have certain knowledge. The earliest of which we have certain knowledge! What a confession of ignorance lies in those words, yet what hopes they hold out for geologists of the future. Beyond the *Olenellus* beds, we know of sediments in which traces of life are doubtful; but how many thousands of feet of sediment must have been deposited, of which we *appear* to know nothing. I say *appear*, for in many areas we know of great thicknesses of fossiliferous rock, which has been dubbed "Silurian" or "Cambrian," simply because they are fossiliferous. By further study of these we may be able to carry the record of life a step farther back, and fix another datum from which we may conduct our observations among the sediments of a still more remote past.

And what does the recent stratigraphical work teach concerning uniformitarianism and evolution? Little as yet, but something. So complex are the conditions which determine the lithological and palæontological characters of the strata of the geological systems, that the same set of conditions never recurs on a large scale, and every system has therefore some characteristic. Coal may be formed in the future, but there will be nothing quite analogous to the state of things which prevailed during the carboniferous system, and so with the other systems. By degrees we

shall learn the characteristic notes of each great era, and work out its geography, both inorganic and organic. Only in this way shall we attain slowly to that knowledge of the geographies of all past eras, which is geology.

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